173

AUG 4 1937

Vol. 21, No. 2 August, 1937

PSYCHOLOGICAL REVIEW PUBLICATIONS

Journal of Experimental Psychology

EDITED BY

SAMUEL W. FERNBERGER

UNIVERSITY OF PENNSYLVANIA

CONTENTS

Cerebral States During Sleep, as Studied by Human Brain Potentials: A. L. LOOMIS, E. N. HARVEY AND G. A. HOBART, III 127

Visual Size Constancy as a Function of Convergence: T. G. HERMANS 145

The Influence of Familiarization on Preference: A. H. MASLOW 162

A Technique for the Kymographic Registration of Certain Associated Voluntary Movements: R. M. COLLIER 181

Competitive Behavior in the Albino Rat: W. M. LEPLEY 194

The Verbal Conditioning of the Galvanic Skin Reflex: S. W. COOK AND R. E. HARRIS 202

The Effect of Binaural Occlusion of the External Auditory Meati on the Sensitivity of the Normal Ear for Bone Conducted Sound: N. H. KELLEY AND S. N. REGER 211

Quantitative Correlates of Euphoria: W. A. BOUSFIELD AND H. BARRY, JR. .. 218

An Experiment to Determine How Accurately College Students Can Interpret the Intended Meanings of Musical Compositions: M. RIGG 223

Can the Brainedness Factor Right or Left be Established by Graphic Time Data Alone?: CLARENCE QUINAN 230

Data Bearing on the Conditioning of Voluntary Reactions: J. M. STEPHENS 236

A Multiple-Exposure Tachistoscope: C. E. FERREE AND G. RAND 240

PUBLISHED MONTHLY, TWO VOLUMES PER YEAR
FOR THE AMERICAN PSYCHOLOGICAL ASSOCIATION
BY THE PSYCHOLOGICAL REVIEW COMPANY
PRINCE AND LEMON STS., LANCASTER, PA.
AND PRINCETON, N. J.

Entered as second-class matter, July 1, 1920, at the post-office at Lancaster, Pa.

PUBLICATIONS

OF THE

AMERICAN PSYCHOLOGICAL ASSOCIATION

EDITED BY

S. W. FERNBERGER, University of Pennsylvania (*J. Exper. Psych.*)
WALTER S. HUNTER, Brown University (*Abstracts*)
HERBERT S. LANGFELD, Princeton University (*Review*)
JOHN A. McGEOCH, Wesleyan University (*Bulletin*)
JOHN F. DASHIELL, Univ. of North Carolina (*Monographs*)
GORDON W. ALLPORT, Harvard University (*J. Abn. and Soc. Psychol.*)

HERBERT S. LANGFELD, Business Editor

PSYCHOLOGICAL REVIEW

containing original contributions only, appears bi-monthly, January, March, May, July, September, and November, the six numbers comprising a volume of about 540 pages.

PSYCHOLOGICAL BULLETIN

containing critical reviews of books and articles, psychological news and notes, university notices, and announcements, appears monthly (10 numbers), the annual volume comprising about 720 pages. Special issues of the Bulletin consist of general reviews of recent work in some department of psychology.

JOURNAL OF EXPERIMENTAL PSYCHOLOGY

containing original contributions of an experimental character, appears monthly (beginning January, 1937), two volumes per year, each volume of six numbers containing about 625 pages.

PSYCHOLOGICAL INDEX

(discontinued after No. 42, June, 1936)

is a compendious bibliography of books, monographs, and articles upon psychological and cognate topics that have appeared during the year. The Index has been published annually from 1895 to 1936, the volumes covering the literature of 1894 through 1935. Its bibliographical services will be carried on by the Psychological Abstracts.

PSYCHOLOGICAL ABSTRACTS

appears monthly, the twelve numbers and an index supplement making a volume of about 700 pages. The journal is devoted to the publication of non-critical abstracts of the world's literature in psychology and closely related subjects.

PSYCHOLOGICAL MONOGRAPHS

consists of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. The price of single numbers varies according to their size. The Monographs appear at irregular intervals and are gathered into volumes of about 500 pages.

JOURNAL OF ABNORMAL AND SOCIAL PSYCHOLOGY

appears quarterly, June, September, December, March, the four numbers comprising a volume of 448 pages. The journal contains original contributions in the field of abnormal and social psychology, reviews, notes and news.

ANNUAL SUBSCRIPTION RATES

Review: $5.50 (Foreign, $5.75). **Journal:** $14.00 ($7.00 per volume, Foreign $7.25).
Monographs: $6.00 per volume (Foreign, $6.30).
Bulletin: $6.00 (Foreign, $6.25). **Abstracts:** $7.00 (Foreign, $7.25).
Abnormal and Social: $5.00 (Foreign, $5.25). Single copies $1.50.
Current numbers: **Journal,** $1.25; **Review,** $1.00; **Abstracts,** 75c; **Bulletin,** 60c.

COMBINATION RATES

Review and Bulletin: $10.00 (Foreign, $10.50).
Review and J. Exp. (2 vols.): $17.00 (Foreign, $17.75).
Bulletin and J. Exp. (2 vols.): $18.00 (Foreign, $18.75).
Review, Bulletin, and J. Exp. (2 vols.): $22.00 (Foreign, $23.00).

Subscriptions, orders, and business communications should be sent to the

PSYCHOLOGICAL REVIEW COMPANY

PRINCETON, NEW JERSEY

Journal of
Experimental Psychology

VOL. 21, NO. 2 AUGUST, 1937

CEREBRAL STATES DURING SLEEP, AS STUDIED BY HUMAN BRAIN POTENTIALS

BY ALFRED L. LOOMIS, E. NEWTON HARVEY AND GARRET A. HOBART, III

From the Loomis Laboratory, Tuxedo Park, N. Y., and the Physiological Laboratory Princeton University

INTRODUCTION

The purpose of this communication is to record our observations and interpretations of the marked changes in brain potential pattern of normal individuals in sleep and the variations in potential pattern of these persons awake. Our experience comes from the careful analysis of over 200 records representing 61 individuals, a total of some 600 hours of recording. Eighty-four records of 30 individuals have been taken during sleep. Twenty-one of these were all night records, the remainder were of sleep during the day. In a field which is rapidly expanding and which involves the complex activity of the human brain we feel that the interpretations reached by different groups of workers are particularly important. Space does not permit a discussion of all previous work. The earlier literature is given in our first communication (Loomis, Harvey and Hobart, 1936) and we list only the more recent papers dealing directly with the subject. Sleep has been studied by Gibbs, Davis and Lennox (1935), by Jasper (1936) and by Blake (1937) in humans; by Bremer (1936, 1937) in animals. The physiology of sleep is covered in an excellent review by Kleitman (1929).

Largely by the development of a type of amplifier system specifically designed to faithfully record the unusual types of potential occurring during sleep, we have been able to establish very definite *states of sleep* which change suddenly from time to time, and to correlate these with movements, with dreams, and with external stimuli applied to the sleeping subject.

Technique

The general recording arrangements for taking long continuous ink records on an 8 ft drum are similar to those described in our previous paper. The three new sets of amplifiers, installed during the year, were designed to give stable and constant amplification over long runs and to have a long 'time constant,' in order to record faithfully relatively slow changes in the potentials. They were also designed not to block when large surges occur. Each of the three circuits is completely independent so that the subject need not be 'grounded' in any way. Each circuit consists of a 4-stage push-pull amplifier which has certain special devices referred to below. The amplifiers are provided with a proper amount of 'negative feed back' so that undesired or in-phase signals are amplified only to a very small extent, while the desired signals that appear differentially on the imput grids of the push-pull amplifiers may be amplified to the desired amount. This negative feed back provides for great stability and constancy of amplification. We have also found that placing large resistances directly in series with the grids of the last two stages reduces the 'blocking' very considerably, since these grids, when driven positive by too large surges, cannot draw much current. This device has recently been described by Schmitt (1937). Any one of the three amplifier systems can be tuned to any desired frequency. The outputs of the three amplifiers are led to three cathode ray oscillographs and to the imputs of the three power amplifiers which operate three specially built dynamic ink writers. In order to limit the signal that can be applied to the ink writers so that they will not be damaged by an excessive signal, a limiting device has been found extremely satisfactory. This limitor consists of a neon light connected across the plates of the fourth stage, which discharges when the signal voltage reaches approximately 90 volts.

With three pens the recording drum runs for 3½ hours before the paper has to be changed. The drum, 44 inches in circumference, makes one revolution in exactly one minute and therefore no additional time scale is required. Auxiliary devices permit the sending of various stimuli to the subject; for example, sound, light, touch, electric stimulation, etc., in any desired combination, and the drum is arranged to send these signals automatically at pre-determined parts of each minute. A fourth pen records on the drum when any stimulus is sent. It can also record heart beats or respiration or noise.

Electrodes and Electrode Position

The electrodes are made of fine silver wire bent into spirals 5 mm in diameter. They are covered with Sanborn electrode paste and held in position on the scalp by collodion. No cutting of hair is necessary. The electrodes remain firmly attached and their resistance is low after 24 hours.

A switching box outside of the bedroom providing for eight leads from the subject allows any combination of eight positions to be connected to any one of the three amplifiers.

For convenience of reference we will refer to the following positions by number as they have been the positions most commonly used:

1. Above and to the left of left eye.
2. In mid line on top of head over motor area.
3. In mid line on occiput.
4. Behind left ear.
5. Behind right ear.

We have frequently used the following three combinations of electrodes for sleep studies. The 4–1 combination records eyeball movements and frontal potentials; the 4–2 combination records from the motor region where the spindles (14 cycle rythm) appear. We often have this amplifier tuned to 14 cycles. The 4–3 combination records alpha waves (10 cycle rythm) from the occiput. We have also used the triangular combination 2–3, 3–4, 4–2.

Normal Variation

Any discussion of the electrical potentials of the brain during sleep should be based on a knowledge of the normal variations to be expected in different individuals while awake. In addition to the general examination of 61 persons, we have studied 26 adults, 13 male and 13 female, in a special series of exactly similar tests with similar positions of the electrodes, the experiment in each case taking about two hours and being divided into ten sections.

There were three identical sections at beginning, middle and end of experiment. The purpose of these sections was to see if any change in the normal pattern, especially in the alpha rythm, occurred during the experiment, that might be interpreted as a disappearance of nervousness or apprehension on the part of the subject. In these sections the subject was lying quietly in a dark room with eyes closed listening to Ravel's Bolero from a phonograph record. Estimation of the amount of alpha waves appearing was facilitated by the use of the 'integrator,' previously described (1936) which automatically sums and records each half minute the amount of alpha rythm appearing during the preceding half minute. It was observed that the alpha rythm did become more marked in some persons toward the end of the experiment but in others there was no change.

In section two, the subject, in a dark room, was asked to open the eyes on a tone signal lasting 10 seconds and then close them when the tone ceased. This signal was repeated

at half minute intervals. After a time a light was flashed in the room simultaneously with each tone signal for three or four times, then came tone signals again without a light. The purpose was to test the effect of light on the potentials and to see if any 'conditioning' occurred, *i.e.*, to see if the alpha rythm, which is suppressed by light, would also be suppressed by tone alone following a previous experience of tone and light. It was found that this conditioning is marked in some persons and not in others.

This section was followed by others to test the effect of calling up visual images, the influence of reciting poetry, of attention to the movement of a faint spot of light, of listening to a just audible sound, of listening to and trying to influence the subject's own alpha rythm converted into sound in a loud speaker, and of listening to sharp staccato sound pulses (sound flicker). Finally a test was made to see if flicker potentials appeared as the result of a strong light flicker on an opal glass globe before the open eyes of the subject.

We observe, as have all others working in this field (Davis and Davis, 1936; Lindsley, 1936; Travis and Gottlober, 1936; Jasper, 1936), every possible gradation between the type with marked, very regular continuous alpha rythm and the type with no alpha rythm whatever but with an extremely straight line record with little potential over 10 microvolts amplitude. In one unusual record there was no alpha rythm at any time except just after closing the eyes, when short (one second) 'bursts' (12.5 per second frequency) appeared whether the subject was in the light or in the dark when closing the eyes.

Repeated experiment with the same individual has convinced us as well as others (Davis and Davis, 1936) that the type of record is characteristic of the individual, perhaps an inherited, at least a fixed peculiarity, for we have observed no situations which can definitely convert a non-alpha into an alpha type, although, as is well known, there are a number of conditions besides sleep and light which will suppress the alpha rythm.

Other peculiarities of individual records are as follows:

(1) Opening the eyes in complete darkness will suppress the alpha rythm in some persons. Opening the eyes in the light always diminishes, practically always completely suppresses this rythm.

(2) Return of the rythm after closing the eyes (whether previously in light or in dark) may be gradual or may be sudden and pronounced.

(3) In some persons more and larger alpha rythms appeared from the top or front of the head than from the back. In some persons bursts of alpha rythm from front and back are synchronous, in others they were almost alternate.

(4) In general fewer alpha rythms appear during the test for visual imagery than while reciting poetry, but the effect is not marked, and varies with the individual.

(5) Intently watching the movement of a faint light almost always completely suppresses the alpha rythm. A few alpha rythms may appear in some individuals in the light.

(6) Intent listening for a barely audible sound to occur almost always increases the abundance of alpha rythm; it never decreased it, as compared with any of the other sections.

(7) The sound flicker section gave no indication of a marked brain potential appearing as a result of the pulses of sound.

(8) When the subject was listening for a just audible sound, a loud, unexpected and unusual sound (a nail dropped in a tin bucket) stopped the alpha rythm for a time in some individuals (the 'startle' response), but not in others.

(9) The flicker potentials, which result from a flickering light in the eyes, were readily obtained with only eight of the 26 persons. Seven more showed some indication of potentials synchronous with the flickering light. In one individual with marked alpha rythm the flickering light had the following effect. The rythm was entirely suppressed when the flicker frequency was below 8 per second, or above 17 per second. Between these limits his normal alpha rythm (10.5 per second) was very marked but did not change in frequency as the flicker frequency was varied between these limits. This same type of effect we have observed in other individuals.

(10) The subjective analysis of the person's own brain waves reveals that by ordinary lines of thought the individual cannot influence the alpha rythm but an interesting situation appeared in one subject who was given an arithmetical problem to solve. He noticed that his alpha rythm stopped until he had decided upon the particular method of solving the problem but before the answer was actually obtained. We have frequently noted that a 'feeling of perplexity' will suppress the alpha rythm rather than marked directing of the attention to a particular thing. The element of indecision, or hesitation, appears to be concerned.

Thus it will be observed that many peculiarities of a person while awake appear as definite changes in the brain potential pattern and that these are largely changes which involve the alpha rythm.

Too much stress cannot be laid on the normal variability of individuals and the difficulty of interpreting the patterns of the abnormal from few or short records. If we divide persons into two great groups, the alpha and the non-alpha, it is an intriguing problem to correlate the extreme types of record with some characteristic of the personality, the mental or emotional make-up of the individual. Suffice it to say that we have not yet been able to unequivocally make any such correlation, the difficulty residing not in evaluation of the record but in estimation of the individual. Certainly there is no correlation to high or low intelligence, to good or poor memory.

But besides the mere appearance or non-appearance of the alpha rythm when a subject is lying quietly in the dark, there are many other criteria that may be used and which appear to be specific to an individual, for example, the frequency of the alpha rythm, the time required for the rythm to cease and reappear on a light stimulus, the type of response to 'light flicker,' the amount and appearance of higher frequency rythms. These may some day also be correlated with types of personality.

Besides the alpha rythm there are a number of other potential patterns. Sleep has supplied several regular and

easily distinguishable ones which appear to represent various states or levels of sleep.

States of Sleep

In our earlier studies on sleep (Loomis, Harvey and Hobart, 1935) mention was made of the spontaneous appearance of 'trains' of alpha waves lasting a short time, of the 'spindles' of 14 per second frequency lasting one-half to one second, and of random potentials corresponding to the large 2 to 3 per second rythm of other observers (Gibbs, Davis and Lennox, 1935). "The impression was gained that a change in the level of consciousness was connected with this change in type of wave." "In a night record certain hours of sleep show many 'spontaneous' bursts of waves while other hours show relatively few." In addition the general pattern of record seems to reflect a change in the activity of the cortex as a whole, whereas the 'trains' might appear in different regions depending on the individual. Blake (1937) has correlated brain potential pattern and depth of sleep.

Our subsequent experiments, especially those with the improved amplifier systems, have enabled us to very definitely connect certain potential patterns with states or levels of sleep. For the character of these patterns see our 1936 paper. In the alpha type of person five can be distinguished, described in the sequence in which they appear upon going to sleep, as follows:

A—Alpha. Alpha rythm appearing in trains of various lengths. The eyes may be slowly rolling, under closed eyelids, as indicated by an electrode over the eyebrow.

B—Low voltage. A quite straight record, with no alpha rythm and only low voltage changes of potential. Rolling of the eyes may occur.

C—Spindles. Line slightly irregular with 14 per second spindles of 20–40 microvolts every few seconds.

D—Spindles plus random. The spindles continue together with large random potentials 0.5 to 3 per second. The random voltages may be as high as 300 microvolts.

E—Random. The spindles become inconspicuous, but the large random potentials persist and come from all parts of the cortex. 'Random' includes irregular as well as occasionally fairly regular bursts of slow large potentials. The group of patterns which we now classify under 'random' may be subdivided upon further study. In the 'B' and 'C' states we have occasionally observed bursts of 4–5 per second potentials.

The states are not due to position of the person in bed, for during an afternoon nap in which the subject lay continuously on the back, four different states appeared and during a night's sleep a change from one state to another may occur without any movement whatever.

Our experiments have not been definitely directed to locating the region from which the various types of potentials come, but the alphas are generally of greatest amplitude from the occipital region but may be greater on top or even from the front of the head; the spindles are most marked from the top and the random comes from all parts of the head. The random waves have been particularly stressed by Gibbs, Davis and Lennox (1935), Jasper (1936) and Hoagland (1936) as occurring during sleep and conditions of impaired consciousness.

The non-alpha type of individual passes into the same states of sleep as the alpha type but it is much more difficult to distinguish states *A* and *B*. State *B* is characterized by less high frequency potentials. In general the non-alpha type of record looks much alike, awake or asleep, until the later stages of spindles or random appear.

In persons with intermediate types of record the *B* state is characterized by less potential activity and when disturbed shifts to an *A* level similar to the pattern when awake. Spindles and random states are also present.

One very clear fact has appeared from all sleep records, the continual shift of a person from one state of sleep to another. The changes may be very sudden or they may be gradual, especially from *C* to *D* and *D* to *E*. In Figs. 1, 2 and 3 these

shifts are plotted for a number of persons of the alpha and of the non-alpha type. The *major* movements of the individuals are also marked by crosses.

State of Sleep and Movement

It will be observed from Figs. 1, 2 and 3 that many move-

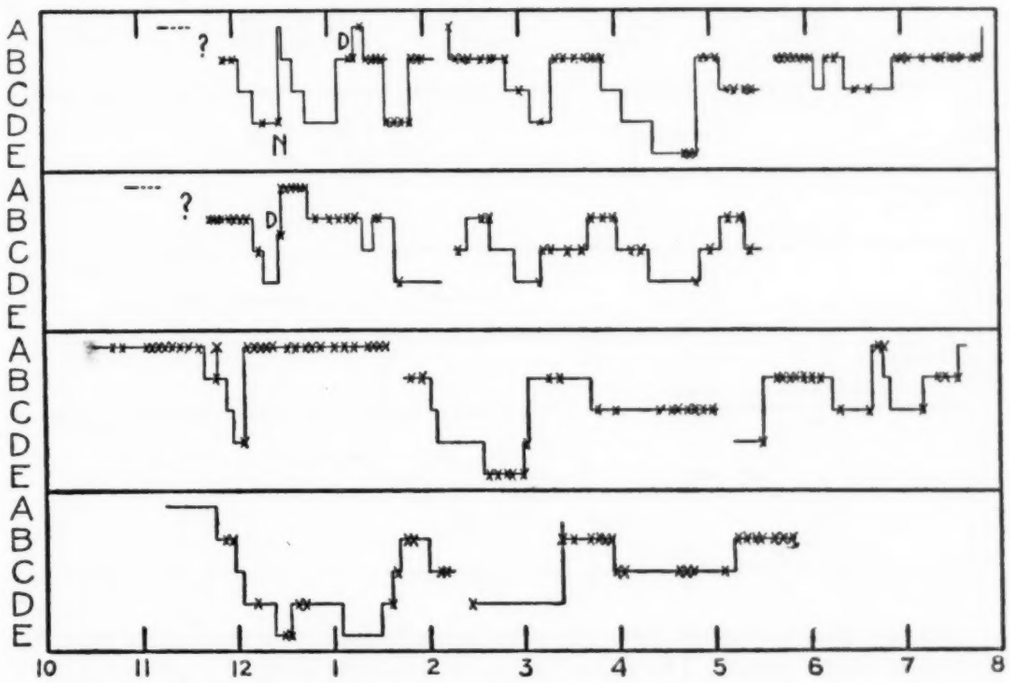


FIG. 1. Night sleep records of four individuals, the first two non-alpha type, the last two alpha type showing changes in states of sleep, *A*, *B*, *C*, *D*, *E* (vertical) during the hours of the night (horizontal). The crosses represent major movements. *N*, noise, *D*, a dream.

ments occur without any change in state of sleep and a change in a state of sleep may occur without movement and with no sound recorded by our very sensitive microphone. However, careful inspection of the records has revealed that changes in state are very frequently connected with a movement. The change after movement is usually upward. A movement in the *B* state is almost always followed by an immediate brief appearance of alpha rythm (*A* state) for a few seconds; occasionally a large long movement may result in alpha rythm for 30 to 60 seconds. Only the latter are shown in Figs. 1, 2 and 3.

Even in the *D* state a large movement may shift the

potential pattern momentarily to the *A* level. Occasionally a movement will be followed by a change in state downward.

There have also been instances in which change from lower to higher states occurs spontaneously but was quickly followed

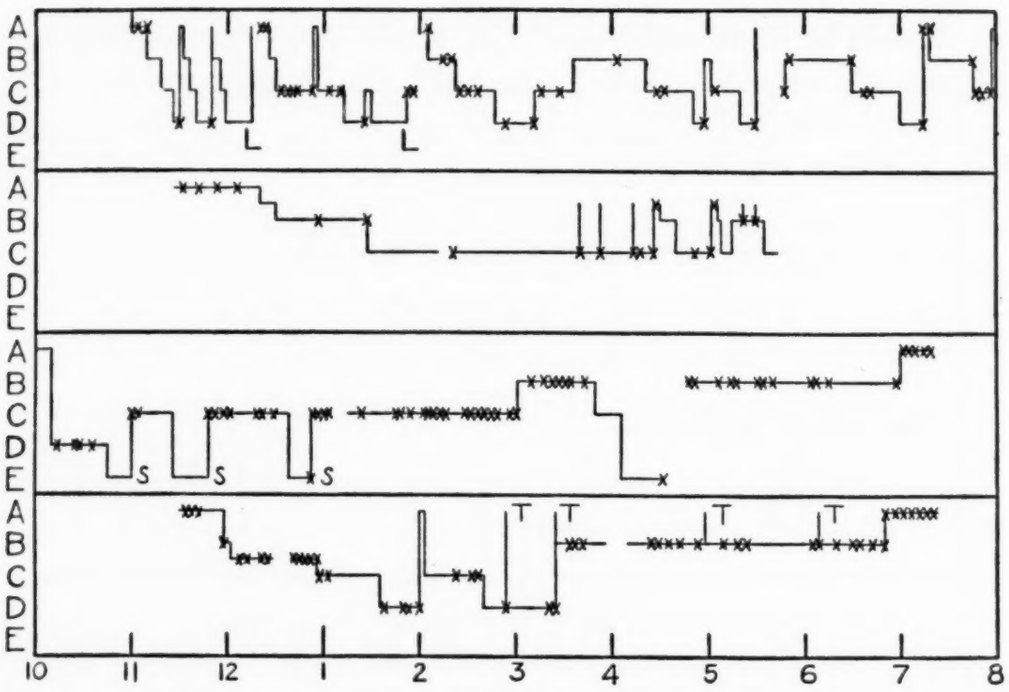


FIG. 2. Night sleep records of four individuals, the first a semi-alpha type, the other three alpha types. The second record is alcoholic slumber. *A*, *B*, *C*, *D*, *E* (vertical) represent states of sleep during the hours of the night (horizontal). *L*, light stimulus. *S*, shaking bed. *T*, subject awoke and noted time. The crosses are major movements.

by a movement. Thus in one case while in the *B* state, continuous alpha rythm appeared ten seconds before a movement and in another case in the *D* state a sudden change to *B* occurred 1 minute before a movement which in its turn was preceded by 20 seconds of alpha rythm (*A* state).

It is quite obvious that the old idea of a continuous curve of sleep needs modification. *Cf.* Kleitman and Mullin (1936–1937).

MOMENT OF SLEEP

The question immediately arises as to the exact moment of sleep in relation to the brain potentials. There is no doubt in our mind that a person in the *B* state is definitely asleep, so that sleep begins somewhere in the latter part of the *A* state

but the exact moment will depend on the criterion of sleep. If the subject is asked to respond, if awake, to a tone signal sent in every minute, we often observe the *A* state to continue for 5 to 10 minutes after the subject has ceased to respond to the signal. Usually the reaction time before the subject has ceased to respond increases, sometimes a response will be

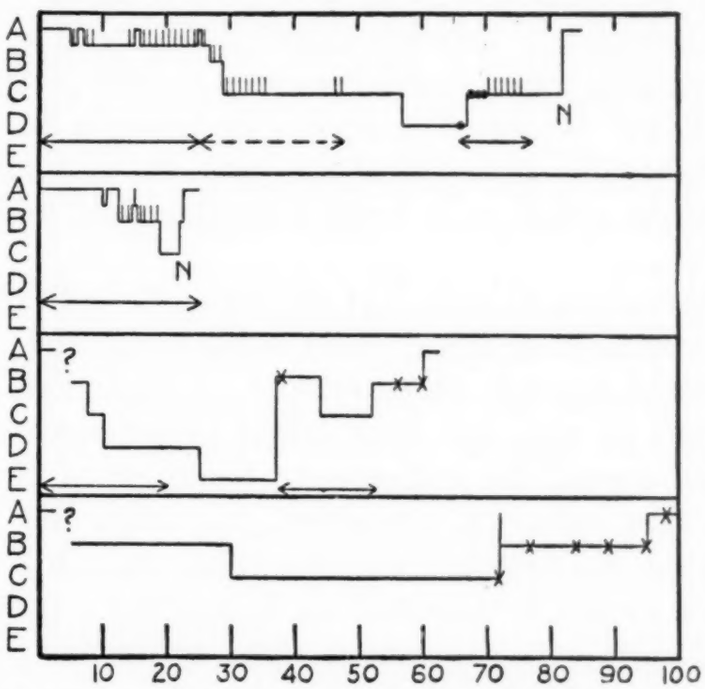


FIG. 3. Afternoon nap records of four individuals, the first two alpha types, the last two non-alpha type showing changes in state of sleep, *A*, *B*, *C*, *D*, *E* (vertical) during minutes of sleep (horizontal). The crosses represent major movements. *N* a loud noise. The horizontal solid arrow line at bottom of each record shows the time a tone and light signal (lasting 5 seconds) was sent in each minute; the broken arrow line where only tone signal was sounded. The short vertical lines represent short bursts of alpha rhythm resulting from tone, changing the person to the *A* state, while the dots are marked bursts of spindles from a tone disturbance.

omitted, to reappear again. We have frequently observed a person to respond when the tone stopped instead of when it began. In any case, such a criterion is a purely arbitrary one and will depend on the intensity of the sound or the quality of the signal, *i.e.* its mental content in relation to the previous experience of the individual.

In one experiment a subject with marked alpha rythm prepared to sleep and was asked to open his eyes on a tone and light (neon lamp) signal (lasting 5 seconds) repeated

every minute. An electrode over the left eyebrow recorded any movement of the eyeball. It was observed that for the first five minutes a suppression of the continuous alpha rythm occurred each time the light was on, but after the first five minutes the alpha rythm became less and less marked between signals. During this period the signals produced marked bursts of such rythm. After a while the alpha rythm disappeared completely between signals. It would appear that the subject had then passed into the *B* state between each signal and that the signal momentarily brought him to the *A* state and that the presence of the light was not sufficient to stop the alpha rythm as it would have done had he been fully awake.

It is interesting to note that the electrode indicated an eye movement with each signal but the light was strong enough to be detected through closed eyelids, so that whether eyelids were opened or not, the light failed to suppress the alpha rythm. However, the subsequent behavior of the subject indicated that light was perceived, for when the light was later omitted and only the tone sounded, the subject, now in the *B* state, evidently detected the lack of light, for he passed into the *A* state with continuous alpha rythm for 50 seconds. This is an example of a phenomenon we have repeatedly observed, namely a change in state upward resulting from lack of a stimulus (the light) previously applied. This experiment shows the difficulty of determining the "moment" of sleep.

Stimuli and Sleep

The shift in states of sleep as a result of stimuli sent to a sleeper offers a field of surpassing interest. It is well known that most sensory pathways are open during sleep, especially the auditory pathway, and that the response will depend on the content rather than the intensity of the sound. Perhaps we may say that a loud sound awakens not because it is loud but because it is unusual, just as a weak sound (a faint cough) may awaken because it is unusual in that it denotes the presence of a person in the room.

We have used sounds as stimuli largely because they are

convenient, but a touch stimulus or a light or lack of light shined on the closed eyelids will also act as a stimulus. In a deaf-mute, tapping the bed brought the subject from the *C* to the *B* and then to the *A* state. We will use the word disturbance as a general term for these various kinds of stimuli.

First, it must be emphasized that changes in states of sleep downward or upward may occur without any detectable external disturbance *i.e.* without a movement or without sound or light stimulus. Internal stimuli undoubtedly play an important role.

Second, we have noted that even in the earlier stages of sleep, sounds will abolish faint snoring without producing a change in the record. This is similar to the effect of sounds in changing the type of respiration during sleep, as reported by Kleitman (1929).

Third, if a disturbance changes the state of sleep the change is always upward. It may be from one state to the next above or one or two states may be skipped. Lack of a stimulus to which the sleeper is accustomed may act as a disturbance. A mere variation in a combination of stimuli may have the same effect.

Fourth, the ease of changing a state of sleep depends on which state the subject is in, changes appearing less and less frequently as the sleeper approaches the *E* level.

Fifth, changes in state as the result of disturbance are frequently but not always accompanied by movement.

Sixth, changes in pattern of brain activity after a stimulus are not the direct representation in the cortex of impulses from that particular sensory pathway but represent changes in state of cortical activity over wide areas. This is particularly evident in sleep.

These generalizations are exemplified by specific examples from some of our records. A sleeper who was in the *B* and *C* states was repeatedly brought into the *A* state for a few seconds each minute by a tone signal sent in at minute intervals. Each change to the *A* state gave rise to bursts of alpha rythm, these bursts becoming shorter and shorter as the *C* state appeared, and ceasing later in this state. Eye

movements also occurred regularly when the tone went on and when it went off, but when the *C* state was reached no eye movement occurred when the tone went off, but still appeared each time the tone went on. We interpret these eye movements (probably under closed eyelids) as repeated attempts of the subject to carry out the instructions received before he went to sleep, *i.e.* to open his eyes whenever the tone sounded and to close them when it ceased. Starting the tone in the *D* state abolished the random waves very quickly (50 seconds) and the sleeper passed to the *C* state and then suddenly to the *B* with bursts of alpha rhythm (*A* state) each minute when the tone sounded. Later a sharp staccato sound was made and the subject reported 'awake.' Continuous alpha rhythm returned. This record is plotted at the top of Fig. 3 and is typical of the stages observed in an afternoon nap and the effect of stimuli when asleep. Awakening in general is sudden although subjects may shift to the *B* or *C* states and back to *A* again some minutes before they actually speak. We are all familiar with the desire to move or speak but with the lack of will power to act when just awakening.

A record of shorter duration of another individual is also plotted (the second in Fig. 3). This may be termed 'unexpected sleep' for the subject, a very sleepy yet conscientious person trying to stay awake, was carrying out the same set of instructions, namely, to open eyes on a tone and light signal and then to close them when the signal ceased, repeated each 30 seconds. A return to the *A* state for 2 to 3 seconds, both when the tone went on and also when it went off, was observed while the subject was in the *B* state. In the *C* state each tone tended to suppress the spindles for 15 seconds, indicating a repeated change back and forth between the *B* and *C* states. In this record and another we have observed the change to the *A* state (alpha rythm) to anticipate the tone signal several times in succession by several seconds as if the subject's cerebral processes had synchronized with the regularly repeated tone.

It has been our experience that all persons with alpha rythm will shift from the *B* and sometimes from the *C* to the

A state as a result of appropriate disturbances; that intermediate alpha types show increased activity and non-alpha persons give no marked potentials under the same conditions.

The time for appearance of alpha rythm after disturbance in the *B* state may be very short, from 0.2 to 1 second with an average of 0.6 seconds. On the other hand, in one case where a tone of a certain pitch was sounded every 10 seconds and the pitch was changed to a higher frequency, the subject sighed and alphas appeared only after 20 seconds. On being awakened the subject did not remember a change in pitch.

When in the *C* state one person, in response to a stimulus, moved, mumbled the word 'awake' and changed to the *B* state, but did not remember it afterwards. Another in the *C* state with tone and light stimulus (for 2 seconds each 10 seconds) passed immediately to the *B* state when the stimulus was no longer sent, whereas later when the subject was in the *D* state no change in the record occurred under similar circumstances.

In the *D* or *E* states most subjects give no response or change of record to ordinary sounds. One doctor of medicine who had been up late several nights gave no change in record to high or low tones, staccato noises or even to the words 'doctor come quickly' spoken over the loud speaker. Another subject, a boy 9 years old, went to sleep so quickly that he was in the *D* level within five minutes. Tones, sirens, entering the room and talking to him, did not change the record and even by shaking him he could only be shifted to the *C* level. (See third record of Fig. 2.)

On the other hand, one sleeper in the *D* state gave no change after two loud and unusual sounds, but 3 seconds after the third sound he moved, spoke incoherently and the pattern shifted to the *A* level. Asked in the morning if he remembered anything unusual, he said no, but on specifically describing the sound he said he did vaguely recall it. We have frequently observed that subjects do not recall the happenings of a night until they are reminded of them. This subject was a radio engineer and the unusual sound was a regenerative squeal in the sound amplifier, which no doubt had a decidedly disagreeable mental content.

Another sleeper while between the *D* and *E* states changed suddenly to the *B* state 10 seconds after a light was flashed in the room and the *A* state appeared 30 seconds later. The subject then groaned, moved and awoke.

Dreams

In our previous paper we have described "a peculiar wave of large amplitude lasting from two to four minutes at a time when no movement or change in heart beat or respiration occurred" and suggested that such a disturbance might be due to a dream. We are now inclined to believe that dreams are not likely to be associated with any unusual pattern of electrical potentials but with a state of sleep. Fortunately we have been able to correlate a dream with the *B* state of sleep in two instances where the subject awoke shortly afterwards.

In one case the sleeper was in the *D* state, changed to the *B* suddenly and spontaneously without a movement, remained in the *B* state for eight minutes and then moved. Three minutes later the subject reported a dream (having a cut finger dressed in a hospital) with the impression that the dream occurred just before the movement.

In the other case the door knob of the sleeping room was turned by one of us. The subject, in the *D* state, immediately changed to the *B* state for one minute, then awoke and reported a dream in which someone had made a noise like a train. No doubt the turning of the door handle was responsible for this. The dreams are indicated by *D* in Fig. 1.

Another sleeper reported next morning a number of dreams but could not remember what they were. The record was unusual in that this person had remained in the *B* state for $3\frac{1}{2}$ hours before awakening, occasionally arousing sufficiently to note the time on a pad of paper. These movements are marked by *T* in Fig. 2.

Summary

1. A new type of push-pull amplifier system, especially designed to record accurately the large slow potentials, is used to study the potential patterns of sleep.

2. Normal variability of different individuals are described together with the changes in potential pattern as a result of various influences.

3. Five states of sleep characterized by strikingly marked differences in potential can be recognized. They are *A*, alpha; *B*, low voltage; *C*, spindle; *D*, spindle and random; *E*, random; in order of appearance and in order of resistance to change by disturbances.

4. The 'moment of sleep' will depend on the criterion of 'sleep.' As sleep begins the record has the character of the *A* state.

5. Movements may occur without a change of state and a change of state without movement, but frequently movement is immediately followed by a change of state upward, occasionally downward, and occasionally a movement occurs just after a change of state.

6. During sleep there is a continual shift in states upward and downward sometimes associated with recognized stimuli, sometimes without any external stimulus but probably as a result of internal stimuli. Such changes are represented by graphs.

7. Stimuli shift the level upward sometimes skipping one or two states but not necessarily awakening the sleeper. Specific instances are described.

8. Dreams in two instances have occurred in the *B* state.

(Manuscript received June 14, 1937)

References

Bacchi, B. K., The adaptation and variability of response of the human brain rythm, *J. Psychol.*, 1937, 3, 463–485.

Blake, H., Brain potentials and depth of sleep. Amer. Physiol. Society Abstracts, *A. J. Physiol.*, 1937.

Bremer, E., Cerveau 'isole' et physiologie du sommeil, *C. r. Soc. de biol.*, 1935, 118, 1235–1241; 1936, 122, 460–467.

Bremer, F., L'activite cerebrale au cours du sommeil et de la narcose, *Bull. de Acad. Roy. de Med.*, 1937, 68–86.

Davis, H., and Davis, P. A., Action potentials of the brain in normal persons and in normal states of cerebral activity, *Arch. Neurol. and Psychiat.*, 1936, 1214–1224.

Davis, H., Some aspects of the electrical activity of the cerebral cortex, *Cold Spring Harbor Symposia*, 1936, 4, 285–288.

Gibbs, F. A., Davis, H., and Lennox, W. G., The electro-encephalogram in epilepsy and in conditions of impaired consciousness, *Arch. Neurol. and Psychiat.*, 1935, 34, 1133–1148.

JASPER, H. H., Cortical excitatory state and synchronism in the control of bioelectric autonomous rythms, *Cold Spring Harbor Symposia*, 1936, **4**, 320–338. Also *Science*, 1936, **83**, 259–260.

HOAGLAND, H., Pacemakers of human brain waves in normals and in general paretics, *Amer. J. of Physiol.*, 1936, **116**, 604–615.

HOAGLAND, H., Some pacemaker aspects of rhythmic activity in the nervous system, *Cold Spring Harbor Symposia*, 1936, **4**, 267–284.

HOAGLAND, H., Electrical brain waves in schizophrenics during insulin treatments, *J. Psychol.*, 1936, **3**, 513–519.

KLEITMAN, N., Sleep, *Physiological Reviews*, 1929, **9**, 624–665, and later papers in *Amer. J. Physiol.*

KLEITMAN, N., AND MULLEN, F. J., Is there a continuous curve of the depth of sleep?, Amer. Physiol. Soc. Abstracts, *Amer. J. Physiol.*, 1936–1937.

KNOTT AND TRAVIS, A note on the relationship between duration and amplitude of cortical potentials, *J. Psychol.*, 1937, **3**, 169–172.

KREEZER, G., Electric potentials of the brain in certain types of mental deficiency, *Arch. Neurol. and Psychiat.*, 1936, **36**, 1206–1213.

LEMERE, F., The significance of individual differences in the Berger rhythm, *Brain*, 1936, **59**, 366–375.

LEMERE, F., Berger's α rhythm in organic lesions of the brain, *Brain*, 1937, **60**, 118–125.

LINDSLEY, D. B., Brain potentials in children and adults, *Science*, 1936, **84**, 354.

LINDSLEY, D. B., AND RUBENSTEIN, B. B., Relationship between brain potentials and some other physiological variables, *Proc. Soc. Exp. Biol. Med.*, 1937, **35**, 558–563.

LOOMIS, A. L., HARVEY, E. N., AND HOBART, G., Potential rhythms of the cerebral cortex during sleep, *Science*, 1935, **81**, 597–598 and **82**, 198–200.

LOOMIS, A. L., HARVEY, E. N., AND HOBART, G., Electrical potentials of the human brain, *J. Exper. Psychol.*, 1936, **19**, 249–279.

MAX, L. W., An experimental study of the motor theory of consciousness, *J. Comp. Psychol.*, 1935, **19**, 469–486.

SCHMITT, O. H., Prevention of blocking in resistance capacity coupled amplifiers, *Rev. Sci. Inst.*, 1937, **8**, 90.

TRAVIS, L. E., AND GOTTLOBER, A., How consistent are an individual's brain potentials from day to day?, *Science*, 1937, **85**, 223–224. See also *Science*, 1936, **84**, 532–533.

TRAVIS, L. E., AND KNOTT, J. R., Brain potentials from normal speakers and stutterers, *J. Psychol.*, 1936, **2**, 137–150.

TRAVIS, L. E., AND KNOTT, J. R., Brain potential studies of perseveration. I. Perseveration time to light, *J. Psychol.*, 1937, **3**, 97–100.

WALTER, W. G., The location of cerebral tumours by electro-encephalography, *Lancet*, 1936, **231**, 305–308.

WALTER, W. G., The electro-encephalogram in cases of cerebral tumour, *Proc. Roy. Soc. Med.*, 1937, **30**, 579–598.

VISUAL SIZE CONSTANCY AS A FUNCTION OF CONVERGENCE

BY THOMAS G. HERMANS

University of Washington

It is commonly reported that the visually perceived size of objects is not consistent with the area of retinal stimulation. For when two or more similar objects different in size are placed at such distances as to yield the same retinal stimulation they still may be perceived as different in size. To make them appear to be visually equal it is usually necessary to place them at such distances that the one actually larger stimulates a smaller retinal area, as geometrically determined, than does the smaller. This phenomenon is known as 'visual size constancy.'

The thesis of the present paper is that the kinaesthetic stimuli derived from the muscular activities in binocular convergence accompanying the retinal stimulation are specific cues determining visual size constancy; that these stimuli are the conditioners of this illusory perception while the retinal concomitant plays only a minor but necessary part in the production of this illusion. This possibility is quite uniformly overlooked in the literature on the subject.

The Current Confusion in Dealing with the Problem of Visual Size Constancy

There are several reasons for this neglect of the factor of convergence in discussions of visual size constancy. Some discussion of convergence as one cue to perception is traditionally given in the usual text-book treatment of visual space perception, but size is mentioned only as another cue to depth or distance perception, and this only in the case of objects of known size. The question is not raised as to

whether convergence is a cue for the perception of size.[1] Probably the major reason for this neglect of convergence in dealing with visual size constancy is attributable to the manner in which Köhler treats the problem in his 'Gestalt Psychology.' This source constitutes the main impetus in English to interest in the phenomenon, yet there is in it no reference to the possibility of convergence as a causal factor.[2] For this reason a careful consideration of this source seems justified. Furthermore, there are certain points in Köhler's discussion of the constancy phenomena that are sources of confusion, and misdirection of effort, if a complete understanding of the facts of size constancy is to be obtained.

The principal source of confusion resides in the fact that Köhler pools the three constancy phenomena—of size, of form, and of brightness—as illusions in visual perception attributable to and exemplary of the same cerebral dynamic relations. "The man approaching us on the street does not grow larger, as for simple optical reasons he should. The circle observed on the oblique plane does not become an ellipse, it seems to remain a circle after its retinal image has become a very flat ellipse. The white object in the shadow remains white, the black paper remains black in full light, although the first may reflect much less light than the second. These three examples have one common property: the physical object as such remaining constant, while actual stimulation varies according to more or less accidental conditions (of distance, of position, of illumination) our experience agrees with the constancy of the physical object much better than

[1] It is interesting to note that Higginson, G. DeV., *Psychology*, MacMillan Co., N. Y., 1936, p. 158, contrary to the rather common agreement that convergence is a cue to depth perception, quotes with approval the physiologist Starling as saying that convergence has little or no effect on the perception of distance.

[2] It is true, however, that Köhler has considered this possibility. Schur, E., Mondtäuschung und Sehgrössenkonstanz, *Psychol. Forsch.*, 1926, 7, 44–80, under the direction of Köhler, in experimenting with the moon illusion considered the efficacy of convergence in attempting to account for the peculiarities of the data they obtained, but discredited its causal efficacy both in the moon illusion and the more general problem of visual size constancy. Another and subsequent instance of Köhler's consideration of convergence will be cited later in considering Koffka's treatment of the problem.

with the varying stimulation produced by it." [3] The implication throughout his discussion of the constancy phenomena is that these three illusions are to be regarded as much the same kind of phenomenon and are to be accounted for in much the same way. The obvious item of difference which is peculiar to size constancy, that of convergence, Köhler disregards.

In fairness to Köhler it should be mentioned that the discussion referred to may not include the complete explanation of visual size constancy that he might have given had his intention been to account for constancy of size in particular. His point, incidentally, is that having arrived at the conclusion that these illusions are modified by meaning, the introspectionist, he claims, is satisfied in dismissing it, but that they should be considered as illustrative of ordinary experience which needs to be accounted for and considered in any discussion of the dynamics of experience. It is the dynamic relations of ordinary experience and not their analysis in which Köhler is interested. This interest is legitimate, but in the case of visual size constancy it has led him to take too cursory a view of the 'whole.' He implies that the introspectionist would contend that the factors of pure sensory experience involved in the problem are only retinal. In so doing he implies that on the sensory side the retinal factors are the only ones which he also would consider. Since he does not return to the problem, one is left to conclude that he is as oblivious to some of the facts of the sensory aspects involved as his hypothetical introspectionist whose theories he is criticizing. We are to conclude from his discussion that these three illusions, of size, form, and brightness, have the common property of being the result of cerebral organization of only retinal stimulation.

It is interesting to note that this conclusion apparently has stimulated Thouless [4] to a bit of research. Upon in-

[3] Köhler, W., *Gestalt Psychology*, New York, Liveright, 1929, pp. 79–80. There are further comments, too numerous to quote, to the same effect found throughout the chapter in which the above occurs.

[4] Thouless, R. H., Individual differences in phenomenal regression, *Brit. J. Psychol.*, 1932, 22, 226–241.

vestigation of individual differences in illusions of size, position, and illumination, he found positive correlations between the three, and a negative correlation between these three pooled, and intelligence. This fact he interprets as evidence of a 'group factor,' and concludes 'that those cues of perception are the cues for this phenomenal regression.' There is no further analysis on his part of the factors involved.

Another point of confusion in Köhler's discussion is his statement that these illusions tend to disappear with a restriction of the cues for perception. "Obviously, if we do not see the distances, the oblique position and the degree of illumination, no reproduction of the 'normal' size, form and brightness will be possible. It is precisely this which happens when the given objects are observed through holes in a screen: since, under these conditions, the definite surrounding properties being given, the actual distances, position and illumination are excluded from vision, and corresponding reproductions are made impossible." [5] This may be true of illusions of form and brightness, but is not entirely true of size. He may be quite right if he intends the hole in the screen to be used monocularly. Restriction of binocular vision to the objects judged as to size, as is possible with lighted apertures in the dark, does not diminish materially the illusion of visual size constancy.

This same confusion in dealing with the problem of size perception when viewing objects through holes in a screen is illustrated in an experiment reported by Maier.[6] His stated purpose was to determine the factors involved in the illusion of the apparent decrease in the size of an object observed through a pinhole. The subjects observed through a pinhole the size of an aperture and then with binocular vision attempted to reproduce the size with an adjustable aperture. He used pinholes of five different sizes, but the reproductions were consistently smaller than the standard. He concludes: "The illusion is largely due to a reduced image on account

[5] Köhler, W., *op. cit.*, pp. 81–82.

[6] Maier, N. F. R., The illusion of size in pinhole vision, *Amer. J. Psychol.*, 1929, 41,291-295. The author does not intend to imply a causal relation between the views of Köhler and Maier.

of the combination of near accommodation with the pinhole between the object and the dioptric system." Maier does not take into account the fact that pinhole vision is monocular vision, while the reproductions were made with binocular vision.

As indicated above (footnote no. 2), Köhler has considered the problem of the efficacy of convergence in size perception. Subsequent to the publication of his '*Gestalt Psychology*' an experiment was conducted in his laboratory by Frank;[7] the data obtained were interpreted as evidence discrediting the theory that convergence is a determiner of size perception. Koffka in his recent book [8] tells of this experiment. His discussion and evaluation of the experiment exemplifies further the current confusion in dealing with the problem in question. It merits consideration.[9] Koffka has a theory that articulated space tends to become as large as possible, in relation to which he considers perceived size. "A theory of perceived size may possibly be derived from the theory of perceived space. . . . If articulated space tends to become as large as possible it requires forces to make an object appear near. This theory which I have learned from a discussion with Köhler, suggests that the more energy is consumed in bringing the object near, the less is available for keeping it large. . . . A special form of Köhler's theory has been tested in his laboratory by H. Frank. In ordinary experiments on size constancy the two objects to be compared with each other are alternately fixated. . . . But in changing fixation from the near to the far object 'the tension of the musculature for accommodation and convergence is decreased. If, then, one assumes that the visual field would have to part with some of its energy for the purpose of 'near-innervation' and that this loss of energy leads to a relative shrinking of the fixated object . . . then decrease of the tension of the eye-muscles,

[7] Frank, H., Über den Einfluss inadäquater Konvergenz und Akkomodation auf die Sehgrösse, *Psychol. Forsch.*, 1930, 13, 135–144.

[8] Koffka's, rather than Frank's, account of this experiment is considered here for the reasons that it is the source that will be read by American psychologists, and is more recent in publication, and for this if not for other reasons carries greater prestige; also, the fallacies of reasoning in evaluation of the data, to be indicated, are Koffka's.

[9] Koffka, K., *Principles of Gestalt Psychology*, Harcourt, Brace, New York. 1935.

accompanying 'far-innervation,' *i.e.*, a smaller loss of energy by the visual field, would result in a relative enlargement of the fixated object which would more or less compensate (centrally) for the retinal diminution.' Frank, however, performed quantitative experiments to subject it to a rigid test. A square fixated directly was (successively) compared with a square at the same objective distance observed while fixation was either nearer or further. It was found . . . that a square at a fixed distance appears larger when it is fixated than when it lies behind the fixation point, and smaller than when it lies in front of it. Moreover, the size of the non-fixated square varies with the distance of the fixation point from the observer more or less as accommodation and convergence do, except for the fact that this agreement is better for the near than for the far fixation point. Thus the original assumption seems to be proved apart from the unpredictable and unexplained asymmetry of fixation in front of and behind the square." [10] Koffka goes on to test this supposed proof. He calculates that if the muscular factors accounted for the change in size this change should be greater. He concludes, in regard to Frank's assumption: "For the assumption is tantamount to assuming that the energy that goes into accommodation and convergence compensates exactly for the gain in retinal image. Therefore a constant retinal image should produce a perceived size directly proportional to the fixation distance. In our case the shrinkage should be from 8 to 3.6 cm, whereas in reality it is only from 8 to 7.5 cm. Therefore even though the accommodation-fixation energy may contribute to the constancy effect, it is at best responsible only for a very small fraction of it." With this Koffka dismisses the possibility of muscular factors determining the peculiarities of visual size constancy.

Koffka makes two errors in his analysis of the significance of this experiment. First, he implies not only that in Frank's experiment the energies referred to are receiving quantitative treatment, but that there are quantitative differences in the muscular energies corresponding to the differences in convergence. It is obvious that these energies are not being

[10] Koffka, K., *op. cit.*, pp. 237–238.

measured, and furthermore there are no established facts as to the quantitative changes in energy paralleling changes in convergence. All that is being measured are the objective distances of objects and fixation points, and the differences in size of the objects reported perceptually equal. The experiment neither proves nor disproves any theory of the compensatory interplay of retinal and muscular energies. Secondly, Koffka's conclusion that 'even though the accommodation-fixation energy may contribute to the constancy effect, it is at best responsible only for a very small fraction of it,' is fallacious. The fallacy lies in assuming that if muscular tension energies account for it, the change must be to the extent calculated. Since the change is only slight, he concludes that the muscles' energies have but a slight effect upon this change in size. Koffka's conclusion should have been that a change in muscle tension in changing fixation a certain amount, yields a change in perceived size of a certain amount, which he regarded as small.

Demonstrations of Visual Size as a Function of Convergence

In the present state of confusion in dealing with the problem of visual size constancy no plausible explanation of the phenomenon is found. The fact requiring explanation is the inconsistancy existing between the observer's reported perceptions of size comparisons and the area of retinal stimulation involved. Explanations to be valid must agree with the observed facts, but furthermore, the proffered causes, or factors determining the phenomenon to be explained, must be independently observable.[11] The difficulty with explanations in terms of cerebral dynamics, compensatory interplay of muscular and retinal energies, and Thouless' group factor, is that these supposed factors are not now detectable separate from and independent of the reports by the observers which are to be explained. Two antecedent or accompanying conditions of the phenomenon of visual size

[11] See Guthrie, E. R., On the nature of psychological explanations, *Psychol. Rev.*, 1933, 40, 124–137; or, Guthrie, E. R., *The Psychology of Learning*, Harper Brothers, New York, 1935, Ch. 2.

constancy which can be separately detected are the sizes of the retinal images and the muscular adjustments of the eyes during the retinal stimulation. Any explanation of the phenomenon must take both these sources of stimulation into account. The following demonstrations [12] and incidental experiments serve to illustrate that the phenomenon in question is conditioned upon the muscular factors involved and that the retinal factors are but a necessary concomitant.

I. If one fixates an ordinary stereoscopic card having a geometrical design, with eyes crossed so that the right figure is projected upon the center of the left retina and the left upon the center of the right retina, one can accomplish a stereoscopic fusion of the two figures, and the depth effects thus produced will be the reversal of what would be obtained with the stereoscope. This may be called 'crossed free stereoscopy.' One will be able to see three figures, of course, but the middle one of the three will appear to be solid as a result of the fusion; the figure on either side may be disregarded. The fused figure will appear to be much smaller than either figure when fixated normally. The only variation in conditions of vision that could account for this change in apparent size is the variation in convergence under the two conditions of vision. The slight change in distance for a given eye in looking from one figure to the other is a negligable factor in accounting for the marked change in size.

Crossing the visual axes in this manner is not in the response repertoire of most people. Only 20 of a group of 140 students were able to do this without the use of the end of the nose as a point of fixation. Much of the persuasiveness of the present demonstrations will be lost on those persons who are unable to accomplish voluntarily this crossing. For

[12] The first two demonstrations were suggested through reading Hyslop, J. H., Experiments in space perception, *Psychol. Rev.*, 1894, 1, 257–273 and 581–601. The third and fourth demonstrations were suggested through reading Judd, C. H., Some facts of binocular vision, *Psychol. Rev.*, 1897, 4, 374–389. The facts involved in these demonstration were cited in conjunction with the controversy represented in these articles. Hyslop was 'looking to a central explanation of both distance and magnitude, independent of both peripheral conditions and motor impulses,' while Judd supported an 'association and motor sensation theory of visual space.' Neither author mentioned the phenomenon of visual size constancy as such, but the facts of the demonstrations serve as excellent support of the present author's thesis.

them, however, some appreciation of the facts here reported will be possible if in every case where the cross-eyed technique is required they will use a pencil point as a point of fixation placed at the position where the two axes of vision should cross. Whether the observer is fusing the two in crossed free stereoscopy is adequately checked by whether the observer reports the perception of solidity in the fused figure. Because of the necessity of this check stereoscopic rather than plane figures are recommended for use.

To check my own observation of this change in size the twenty students mentioned above were asked to practice with stereoscopic cards until they could successfully fuse the figures. They then were asked if they observed any change in the apparent size, and all agreed as to the decrease. When asked to select from an assortment of similar figures varying in size, the one representing approximately the size of the fused figure, their selections averaged .7 the size of the standard.

II. If one cuts the stereoscopic card in two, so that the distance between the figures may be varied, an incontrovertible demonstration of the present thesis is made possible. While holding the fused figure in crossed free stereoscopic vision, move the figures apart. This necessitates an increasing convergence, and accompanying it is observed a marked shrinkage in the apparent size; upon moving the figures together the apparent size increases. The few observers who were able to do this agreed as to the change in size. From this demonstration it is still more evident that the muscular factor is in this case the determiner of visual size constancy, for again, the size of the retinal stimulation does not change significantly yet one perceives an astonishing phenomenal change in size.

If one is able to experience this demonstration the problem of perceived distance changes is almost certain to arise. On first observing the apparent shrinkage in size upon moving the figures apart there appeared to the writer to be a distinct approach of the fused figure. Two of the observers noted this same approach so vividly, without being asked anything

about distance changes, that they dodged and then laughed, saying that the figure appeared to move rapidly toward them. Now after much practice in observing this shrinkage the writer is unable to perceive any consistant distance changes, yet the size changes persist.

III. Hold two mirrors in the same plane with their adjacent edges lying in the medial plane of the body, about one foot from the eyes. It is possible with the mirrors in this position to fixate, with the left eye in the left mirror, the image of an object (also in the medial plane) such as a light several feet away, and with the right eye in the right mirror, the image of the same object. If, while fixating the object in this manner, one changes the planes of the mirror surfaces so that the angle between them is increased to slightly more than 180 degrees or to slightly less than 180 degrees, one will note a change in the apparent size of the image. When the change is toward an angle greater than 180 degrees, the image appears to decrease in size, and when in the opposite direction the image increases in size. Increase in the angle of the mirror surfaces increases the convergence necessary to continue fixating the object binocularly, and accompanying this is a decrease in apparent size; decreasing the angle decreases convergence, yielding an apparent increase in size. Visual size is again shown to be a function of muscular adjustments of the eyes. In this case the size of the retinal image does not change; the only change to account for the change in size is the change in convergence. The same data may be obtained with the mirror stereoscope.

IV. If one decreases by about 30 degrees the angle between the surfaces of the two mirrors, moves them apart a few inches, and out at about arms length, one may, by applying the cross-eyed technique, fixate the image of an object with the right eye in the left mirror and with the left eye in the right mirror. Upon doing this there is an apparent decrease in the size of the image, as with the stereoscopic cards. Further decreases and increases in size are obtained upon changing the angles between the two mirror surfaces, thereby necessitating changes in the convergence of the eyes. With greater convergence

there is perceived a decrease in size, and vice versa. This is not attributable to any change in the size of the retinal image; for there is none.

V. Place two similar objects, one twice the dimensions of the other in positions as shown in Fig. 1. The smaller should

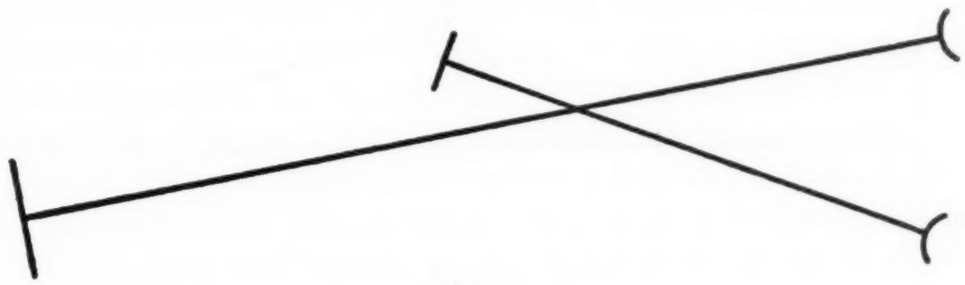


FIG. 1.

be placed at half the distance from one eye that the larger is from the other eye. One could use any two similar objects such as the lighted apertures described in demonstration VIII, but it might be better, for reasons mentioned in demonstration I, to use stereoscopic figure. When the two objects are thus placed and viewed as in ordinary vision, the farther will appear to be obviously larger than the nearer. On crossing the eyes, as indicated in the figure, one can cause the two images to fuse into one; the fusion, however, will appear to be that of two figures the same size and slightly smaller than the near figure. This stereoscopic fusion is not possible when the larger is moved out to a point anywhere near the distance at which with alternate binocular fixation of the two it would appear to be the same size. This illustrates that the retinal image of the two were the same size before crossing the eyes, but that the difference in muscle adjustment in fixating either in comparison determined the apparent difference in size.

VI. Place two lighted apertures, one twice the diameter of the other, in positions as indicated in Fig. 2. The distance of the larger, 2, from the observer at *O*, is twice that of 1, the smaller. Any two similar objects under conditions of ordinary illumination would yield the phenomenon described in the following statement, but one can facilitate the demonstration of the efficacy of kinaesthetic cues by eliminating the other cues ordinarily involved in the perception of size by the use

of lighted apertures in an other-wise dark room. Fixate on lighted apertures 1 and 2, illuminated alternately, points at the left of 1 and the right of 2. The after-images of 1 and 2—

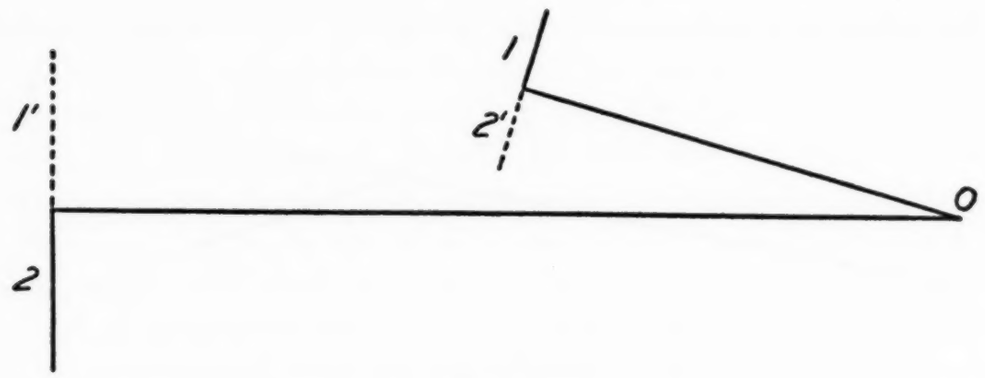


FIG. 2.

1′ and 2′—will appear to be the same size as the apparently adjacent lighted aperture. Furthermore this equality in apparent size of the lighted aperture and adjacent after-image is only possible when the apertures are placed at such distances as to yield retinal images identical in size. The significance of this demonstration will be fully appreciated upon consideration of the following demonstration.

VII. If one fixates the point of a pencil while holding an after-image of some object, the image will appear to change in size upon varying the distance of the pencil. It will appear to shrink upon approach of the pencil and increase in size upon moving the pencil away. Demonstrations comparable to this are usually included in discussions of the apparent size of the after-image. The usual explanation of such demonstrations is that the after-image subtending the same angle in the visual field, regardless of distance, covers a greater area of objects of known size the greater the distance the object is from the observer. It is obvious that in such demonstrations the cues for perception of size other than the kinæsthetic cues from variations in convergence are not eliminated. To utilize the after-image in the demonstration of the efficacy of the kinæsthetic cues alone in determining apparent size, the following apparatus was devised.

Four pieces of one inch brass tubing 1½ inches long were

cut and in each a hole bored on one side ½ inch from the top. The holes were 1, 2, 3, and 4 mm. in diameter. These pieces of tubing were mounted on blocks of wood with a flash-light bulb placed inside at the base of each. Across the holes or apertures were pasted bits of paper to reduce the intensity and diffuse the light. The tubes were capped and electrically wired to a common switchboard and battery, in such a manner that they could be illuminated either simultaneously or in sequence. The intensity of illumination of the bulbs was equated by placing the same resistance in the circuit of each. These lights were placed along a board in such positions that each aperture was visible to the observer at *O*, as in Fig. 3,

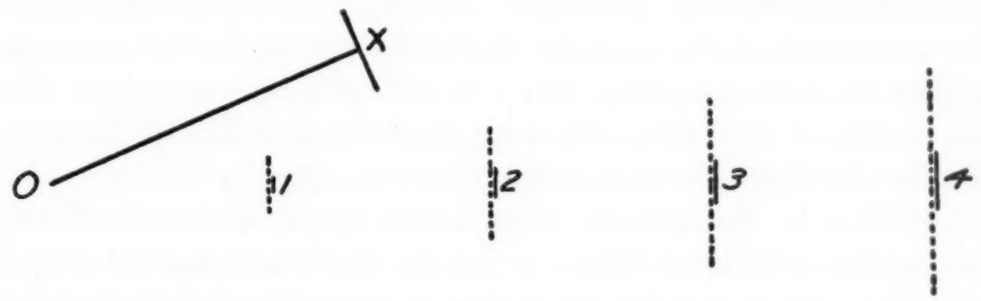


FIG. 3.

and all were at such distances as to subtend the same angle in the observer's visual field. They were all within a range of six feet from the observer, and in the same horizontal plane with his eyes.

When these were illuminated in a dark room they appeared to be graduated in size. This, of course, exemplifies visual size constancy. They are, however, practically point stimulations of light which may serve as points of fixation in an otherwise dark room while judging any change in the apparent size of the after-image. Since each aperture yields the same retinal stimulation there would be no cue from their visual properties for the perception of the apparent size of the after-image, and the fact that the rest of the room is dark, leaves the muscular factor as the only other possible cue determining its size. The object used for the retinal fatigue in the production of the after-image was a circular piece of white paper eight inches in diameter with a small area of the

center cut out. This was mounted on a large back-ground of black velvet. Fixation of the black center saved the fovea from light adaptation, thereby enabling the observer to detect more readily the dimly lighted apertures in the dark.

The several observers agreed that there was an apparent change in the size of the after-image when the apertures were illuminated and fixated in sequence. The change was an increase in size when the sequence of illuminations was from the nearer to the farther apertures and a decrease in size when the sequence was reversed. No quantitative measure of the change was attempted, but from demonstration VI one would anticipate that the change would be found to be directly proportional to the distance. Probably the best technique for determining the amount of change would be the one suggested in demonstration VI. In doing this, one might discover minor variations from an exact proportion of distance to size attributable to accommodation.

VIII. If the retinal stimulation alone determined the perception of size, an object twice the size of a standard object would appear to be the same size at twice the distance of the standard. This apparently is not the case. However, the farther away both objects are from the observer, the less the convergence, and the less the difference in convergence required in the fixation of the two. Conversely, the nearer both the compared objects are, the greater the difference in convergence for both. If convergence determined the phenomenon in question, one would expect it to disappear at some greater distance. The curve of this phenomenal regression, as some have called it, would coincide with the geometrical optical relationship at the greater distances. One would expect also to obtain a curve for monocular vision closer to the geometrical optical relation than the curve for binocular vision.

To test this hypothesis an experiment was performed at night in the University's stadium, where a level straightaway 1000 feet in length was available. The apparatus consisted of two lighted circular apertures, one 4½ inches in diameter and the other 9 inches, cut in the top of 10 inch

cubical boxes. Flashlight bulbs and batteries were used for illumination. The apertures were covered with plain white paper sufficiently translucent to diffuse the light evenly. The stadium was dark except for city lights reflected from overhanging clouds and slight illumination from a section of the city about one mile away across a stretch of water. The equivalent of perfect darkness was approximated, however, by keeping the observers' eyes light adapted. The observers reported inability to see any other objects in the general field of vision of the lighted apertures.

The experimental procedure for the position of the lights was as follows. The smaller aperture, *A*, was placed first at a distance of 50 feet, and the larger, *B*, at some farther distance at which it was obviously larger. *B* then was moved out gradually to a distance at which it appeared equal in size to four of the eight independent observers. The apertures were lighted alternately and *B* was moved only while illuminated. A similar judgment was made from a position at which *B* appeared obviously smaller, with the movement toward the observers to a position of apparent equality as judged by half the group. The average of these two positions was taken as the position at which the two appeared to be the same. Judgments of size at successive positions of *A*, each 50 feet farther than the preceding position, were made in the same manner until the positions of the two approximated the geometrical optical relation that the retinal stimulation alone should give. A second series of judgments in the same manner were made with monocular vision.

The judgments of size made with binocular vision gave data yielding a curve approximating the geometrical optical relationship when *A*, the smaller aperture, was placed at a distance of 400 feet. When *A* was placed at 50 feet, *B* was judged equal at 194 feet; with *A* at 100 feet, *B* was judged equal at 262 feet; with *A* at 150 feet, *B* appeared equal in size at 337 feet. The ratio of distances of *B* over *A* consistantly decreased with successive increases in the position of *A*. With *A* at a distance of 400 feet, *B* appeared equal in size at a distance of 793 feet—slightly less than twice the distance.

The curve derived from judgments made with monocular vision was a much closer approximation to the geometrical optical relationship throughout. With *A* at 50 feet, *B* was judged equal at 114 feet; with successive increases in distance of positions of *A* up to 250 feet, the distances at which *B* was judged equal were approximately twice that of *A* . . . approximating a straight line regression. It is apparent that restriction of visual cues does not diminish appreciably the phenomenon of visual size constancy except as restriction means a restriction to monocular vision instead of binocular vision.

Since Helmholtz it has been customary to account for the peculiarities of this curve in terms of binocular parallax. That this factor may be involved in the present experiment is not denied, but it is of negligible importance. Hillebrand's famous alley experiments cited by Helmholtz [13] used series of pairs of rods, all of which were visible; obviously, in such a case, the factor of the binocular parallax would be prominent. The lighted apertures, *i.e.*, plane surfaces, used as described above would certainly minimize if not eliminate this factor.

It also is not denied that variations in the curve might be obtained, possibly with an equating of the optical size of the standard aperture through-out the series of judgments for the different distances, and with light intensities equated throughout. However, in the light of the foregoing demonstrations, the conclusion one may infer from the present experiment remains valid. That is, visual size constancy, being a function of convergence, disappears at those distances beyond the limits of its efficacy.

That the curve for monocular vision does not coincide with the geometrical optical relationship throughout is readily accounted for by the fact of monocular accommodation and also the fact that convergent movements of the unused eye occur in monocular vision. It may be asked then, why the monocular curve does not approximate the binocular curve if convergence were the determiner of visual size constancy. In answer it need only be pointed out that a necessary con-

[13] Helmholtz, H., *Physiological Optics*, English translation, volume 3, 390 ff.

comitant of binocular vision is bi-retinal stimulation. This point illustrates the fact that the foregoing demonstrations cannot be construed as a death blow to gestalt theory. They illustrate principally the errors gestalt theorists make when they neglect analysis. Thouless' conclusion 'that those cues of perception are the cues for this phenomenal regression' after all may not be so meaningless, but it would seem extremely important in gestalt theorizing to first determine what 'those cues' might be; in speculating on cerebral dynamic relations it might be well to know what factors are dynamically related.

(Manuscript received January 16, 1937)

THE INFLUENCE OF FAMILIARIZATION ON PREFERENCE [1]

BY A. H. MASLOW

Institute of Educational Research, Teachers College, Columbia University

Sheer repetition, leading to familiarity, may result in greater liking for the familiar thing or activity, as well as in better learning. That is, repetition leads to affective as well as cognitive change. Theoretically and actually, this affective change may be in either direction, *i.e.*, in the direction of increasing like, or in the direction of increasing dislike. We shall deal with the former in this paper. We shall use the word 'familiarity' to refer only to the fact of *cognitive change* effected by repetition, and never to refer to the *affective change* wrought by the same repetition. We shall use the phrases 'change of preference' or 'changed like or dislike' to refer to this affective change.

The purpose of this experiment was to investigate the extent and nature of the affective changes, if any, wrought by repetition of the activity or condition in the several situations to be listed below.

I. Procedure

1. Fifteen Barnard College students were recruited from the psychology classes. They were paid for their time. The experimental periods were two hours each for ten evenings, four evenings a week.

2. The general scheme of the experiment was concealed from the subjects until the very end. Its ostensible purpose was to study the effects of distractions, interruptions, and changes of work. The materials used were (1) semantic opinion counts, in which the subjects had to make estimates of the relative frequency with which various meanings of a word occurred; (2) copying sentences from a book; (3) information tests; (4) vocabulary tests; (5) interest and attitude tests; and (6) personality tests. Since the kind of work done makes no difference with respect to the true aims of our experiment, no further description is necessary.

In order to make plausible the ostensible purpose of this experiment various distractions were used, such as reading poetry aloud while the subjects were working,

[1] This experiment was made at the suggestion of Dr. E. L. Thorndike and under his direction. He is not responsible, however, for the form of this paper and its conclusions. This study is one item in a program supported by a grant from the Carnegie Corporation.

walking back and fourth, making various noises, etc., as well as frequent changes in the kind of work they were given.

The detailed day-to-day procedure is presented in the Work-Schedule below. The kind of work and the time allotted to each are listed along with any other details of special instruction necessary in the carrying out of the tests.

The List of Experiments below describes the choices made during the course of the experiment, each choice in a sense being a separate experiment on the influence of familiarity, since one member of the choice is always a familiarized one, and the other one unfamiliar (so far as this experiment goes).

All these tests were introduced as casually and innocently as was possible and in most cases aroused no suspicion in the subjects. Statements on this point were obtained from the subjects after the experiment had ended, as well as a statement of what the individuals thought was the purpose of the experiment. The tests are described in more detail in the section on Results.

All the subjects on the first day were seated in alphabetical order around a single large table and told to keep that seat for the duration of the experiment. In front of each subject were stacked the necessary materials for the session, smock, book, tests, etc. Down the center of the table were piles of rubber bands, paper clips, cards, paper and blotters. Around the walls of the room were hung large, brilliantly colored paintings made by the experimenter with a view to being as attention-getting as possible. A metronome was kept ticking every evening at its slowest possible rate (about one tick a second). These conditions were kept constant until the time came for the choice tests on days 7, 8, 9 and 10 and then changed only as noted, in the Work Schedule. Subjects were instructed to use fountain pens.

After the first hour of work, a ten minute rest was allowed during which two small cookies were given to each subject, the same kind being used until the choice test at the end. As a final step on day 9 the subjects were asked to write down what they thought the purpose of the experiment was. No one guessed correctly.

On Day 10, after the purpose of the experiment had been explained, a list of all the preference-choices was written on a blackboard and the subjects were asked to introspect on each of these choices that they had made. Nothing much was learned from this, except that a few people were sure they had made beauty and euphony ratings as well as like-dislike ratings in Tests 1 and 2 without any reference whatsoever to familiarity. An examination of their records showed, however, an average susceptibility to familiarity in spite of their subjective judgment of lack of such susceptibility.

Some time later a control experiment was run on a group of 13 subjects under conditions as nearly like the experimental ones as possible. Control results will be compared below with the experimental results.

Work Schedule

Day 1

Subjects seated alphabetically, all necessary materials at each desk; all necessary instructions given. Paintings on wall. Metronome started.

1. Estimating frequency of meanings	15 minutes
2. Copying sentences	30
3. Rest. Cookies eaten	5
4. Interest inventory	15
5. Information test	10
6. Vocabulary test	10
7. Foreign names read	5

Each subject was given a smock to wear through the entire experiment. On table in little piles, one for each pair of subjects, are rubber bands, yellow paper, unrulled 3 × 5 cards, paper clips, yellow blotters. Each one chooses a book from which to copy sentences (all modern novels).

Day 2

8. Pictures on screen	5 minutes
9. Copying sentences	10
10. Personality test	20
11. Copy sentences	25
12. Rest. Cookies	5
13. True-false test	15
14. Copy sentences	45

Day 3

15. Estimating frequency of meanings	30 minutes
16. Copy sentences	15
17. Personality test	15
18. Rest. Cookies	5
19. Copy sentences	20
20. Personality test	35

Day 4

21. Pictures on screen	10 minutes
22. Copy sentences	50
23. Rest. Cookies	10
24. Liking for words	10
25. Copy sentences	40
26. Foreign names read	3

Day 5

27. Copy sentences	15 minutes
28. Like-dislike test	25
29. Copy sentences	20
30. Rest. Cookies	10
31. Vocabulary test	15
32. Copy sentences	20
33. Like-dislike test	12
34. Foreign names	3

Day 6

35. Pictures on screen	5 minutes
36. Estimating frequency of meanings	15
37. Personality test	10
38. Estimating meanings	30
39. Rest. Cookies	10
40. Copy sentences	15
41. Like-dislike test	15
42. Foreign names read	3
43. Copy sentences	12

Day 7

44. Copy sentences. Interrupt and say can do only significant portions of sentences if they wish..... 30 minutes
45. Vocabulary tests..... 15
46. Copy sentences..... 15
47. Rest. Cookies..... 10
48. Say they can take any seat they wish
49. Information true-false test. Tell them if they wish they can use T ? F system for answering instead of 1 2 5 8 9..... 20
50. Personality test..... 15
51. Copy sentences..... 15
52. Foreign names read..... 2

Day 8

53. Pictures on screen..... 10 minutes
54. Copy sentences. Announce can have a new book to copy from if they wish. Also can make up original sentences instead of copying from book..... 25
55. Foreign names read..... 2
56. Vocabulary test. May use T ? F instead of 1 2 5 8 9..... 15
57. Rest. Cookies..... 10
58. Estimate meaning frequency..... 60

Day 9

No paintings on wall. Can use pencils piled on table instead of fountain pen. Two kinds of paper clips and two kinds of rubber bands in each pile. Given choice between old color blotter (yellow) and new color (orange).

59. Estimate meaning frequency. Announce can use easier system of rating if they wish by using O^+ instead of very small decimals..... 30 minutes
60. Copy sentences..... 30
61. Intermission. Choice of two kinds of cookie..... 10
62. Test familiar and unfamiliar foreign names. Get ratings for like-dislike and for euphony..... 15
63. Vocabulary test. May use either 1 2 5 8 9 or T ? F..... 15
64. Copy sentences..... 10

Day 10

Both ruled and unruled cards on table. Both yellow paper and blue books. Told don't have to wear smock unless they want to.

65. Test of pictures on screen. Ratings for like-dislike and beauty..... 25 minutes
66. Copy sentences, during which metronome stopped. Few minutes later got vote on how many want it to stay stopped and how many want it started again..... 35
67. Choice of 2 kinds of cookies again

68. Got statements on whether absence of wall paintings was noticed and whether or not they were missed. Asked them to write out what they consider purpose of experiment to be.
69. Explain experiment and its purpose. Got written statements of introspection (if any) on the choices they had made.

List of Experiments

1. Familiar vs. unfamiliar pictures.
2. Familiar vs. unfamiliar foreign names.
3. Choice of wearing or not wearng smock.
4. Familiar vs. unfamiliar rubber band.
5. Familiar vs. unfamiliar paper clip.
6. Choice of presence or absence of metronome click.
7. Choice of different seat.
8. Familiar vs. unfamiliar color of blotter.
9. Familiar vs. unfamiliar 3 × 5 cards.
10. Familiar vs. new books to copy from.
11. Choice of copying whole sentence or only significant part.
12. Choice of copying sentences or writing original sentences.
13. Choice of familiar vs. unfamiliar rating scheme.
14. Choice of yellow paper or blue books.
15. Choice of presence or absence of familiar paintings.
16. Familiar vs. unfamiliar cookies.
17. Choice of pen or pencil.

II. Results

The seventeen separate tests or choice experiments will be discussed in order.

1. *Familiar vs. Unfamiliar Paintings*

A pair of paintings by each of fifteen well-known artists was chosen and uncolored lantern slides of these paintings were obtained. The artists chosen were Giotto, Rembrandt, Vermeer, Hogarth, El Greco, Turner, Cezanne, Gauguin, Van Gogh, Rousseau, Degas, Picasso, Orozco, Leger and Kandinsky. The paintings chosen were the less-known works of these artists. A questionnaire showed that very few of them had ever been seen by any of the subjects. Each pair was chosen by the experimenter and a professor of art so as to be as nearly alike as possible in subject, composition, and style. One of each pair was then chosen at random and thus two series of fifteen slides each were obtained.

One of these sets of fifteen slides was projected on a screen

for four evenings, each slide being on the screen for fifteen seconds. Thus each slide was seen for a total of sixty seconds. The subjects were told to look at each carefully as they might be asked later to describe each painting. On day 10, the choice test was made by mixing the F (familiar) and U (unfamiliar) sets of slides and then projecting them on the screen in random order for fifteen seconds apiece. Two ratings were asked for: (1) for extent of like or dislike aside from all question of sheer beauty, or skill, or technical adequacy; and (2) for sheer beauty, disregarding personal like or dislike. The first rating was made as follows:

− 5 dislike very much
− 4
− 3 dislike somewhat
− 2
− 1
0 neither like nor dislike
+ 1
+ 2
+ 3 like somewhat
+ 4
+ 5 like very much

The second rating for beauty was made as follows:

1 sure it is beautiful
2 think it is beautiful, but not sure
5 neither beautiful nor ugly
8 think it is ugly but not sure
9 sure it is ugly

Each subject's rating for all fifteen F paintings were added to give an F score. His ratings for the fifteen U paintings were added to give a U score. The mean of each of the two series was obtained and the significance of the difference between the F and the U mean calculated. Since the two series were correlated very highly, as might be expected, Kelley's formula for the significance of the difference between two correlated series was used after the correlations were obtained. These data are presented in Table II.

TABLE I

INDIVIDUAL DIFFERENCES IN SUSCEPTIBILITY TO FAMILIARIZATION

	Subject No.														
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Paintings, Like-dislike	44	16	12	20	−2	−2	−15	30	16	21	12	13	17	17	−13
Paintings, Beauty	12	2	2	−1	−8	−2	− 6	12	5	2	1	9	4	7	− 5
Foreign names, Like-dislike	23	13	10	5	3	2	6	14	11	0	10	17	19	7	38
Foreign names, Euphony	5	8	10	7	9	5	− 3	5	1	4	1	8	2	2	0
F vs U choice for all other tests	3	10	8	4	5	7	7	9	7	7	9	6	9	8	11

Each figure represents the difference between the F score and the U score for each of the fifteen subjects. A plus score represents a difference in favor of familiarity, a minus score represents a difference in favor of unfamiliarity.

SAME SCORES IN TERMS OF RANKING

	Subject No.														
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Paintings, Like-dislike	1	7.5	10.5	4	12.5	12.5	15	2	7.5	3	10.5	9	5.5	5.5	14
Paintings, Beauty	1.5	8	8	11	15	12	14	1.5	5	8	10	3	6	4	13
Foreign names, Like-dislike	2	6	8.5	12	13	14	11	5	7	15	8.5	4	3	10	1
Foreign names, Euphony	7	3.5	1	5	2	7	15	7	12.5	9	12.5	3.5	10.5	10.5	14
F vs U choices for all other tests	1	14	9.5	2	3	6.5	6.5	12	6.5	6.5	12	4	12	9.5	15

That affective change in the direction of greater liking as a result of familiarity has occurred may be seen. Of 30 pairs of scores 9 U scores (30 percent) are equal to or larger than the corresponding F score, whereas in the control experiment 54 percent of the U scores were larger or equal. Paintings with which subjects have been familiarized by our technique are liked more and are also considered to be more beautiful. This latter finding, while it might have been expected, is extremely interesting since the instructions for rating stressed objectivity as divorced from personal tastes. In the case of both ratings the difference between the means indicates a trend (critical ratio = 1.0 and 1.2). These results are more revealing when it is considered that the 'familiar' paintings were seen for a total of merely one minute each. The control experiment showed that the F group of paintings was even here rated somewhat better liked and more beautiful than the U group, indicating that the two series of paintings were not as equivalent as we had thought. However, this difference was not nearly as great as the difference in the experiment itself. We, of course, allow for this factor in Table II.

TABLE II

RESULTS OF EXPERIMENT WITH FAMILIARIZED AND UNFAMILIAR PAINTINGS

	Mean	$\sigma_{av.}$	r Experiment	r Control	Diff.	Diff.* Corrected	$\sigma_{diff.}$	Critical Ratio
	Ratings for Beauty							
15 Familiarized paintings....	40.6	2.1						
15 Unfamiliar paintings.....	43.0	2.0	.72	.70	2.5	1.5	1.5	1.0
	(A lower score means a higher rating for beauty.)							
	Ratings for Like-Dislike							
15 Familiarized paintings....	18.3	4.8						
15 Unfamiliar paintings.....	5.9	5.1	.70	.41	12.4	4.6	3.9	1.2
	(A higher score means greater liking.)							

It is of interest to notice that the correlations between F and U series for both beauty and like-dislike are greater in the experiment proper than in the control experiment (see Table II). This is also true for the similar correlations obtained in the next experiment with familiar and unfamiliar foreign names (see Table III).

TABLE III

RESULTS OF EXPERIMENT WITH FAMILIARIZED AND UNFAMILIAR FOREIGN NAMES

	Mean	$\sigma_{av.}$	r Experiment	r Control	Diff.	Diff. Corrected	$\sigma_{diff.}$	Critical Ratio
	Ratings for Euphony							
15 Familiarized names...	38.4	1.9						
15 Unfamiliar names....	42.6	2.1	.90	.27	4.2	4.6	0.9	5.1
	(A lower score means a higher rating for beauty.)							
	Ratings for Like-Dislike							
15 Familiarized names...	13.5	6.2						
15 Unfamiliar names....	1.6	6.9	.84	.57	11.9	7.1	3.8	1.9
	(A higher score means greater liking.)							

* Difference corrected equals experimental difference minus the difference obtained in the control experiment.

2. *Familiar vs. Unfamiliar Foreign Names*

The general technique for this test was the same as that for the paintings test. Thirty Russian names for women were matched into fifteen pairs, the two members of each pair being as alike as possible. One list was read out loud on six evenings by the experimenter, one every 3 seconds and the subjects were instructed to write them down as read, trying to spell them correctly. For the test all thirty names were read (both F and U series) and two ratings were asked for, one for like-dislike, the other for sheer euphony of the names.

These results are presented in Table III. Of 30 pairs of scores only one U score is higher than its corresponding F scores, and one U score and F score are equal (7 percent) whereas in the control experiment the corresponding figure is 42 percent. Familiar names (within the limits of our experimental procedure) are judged more euphonious and are better liked. The critical ratios of the differences between the F mean and the U mean, are, for ratings of euphony, 5.1, and for rating of liking, 1.9, the first being statistically significant, and the second approaching it. Only one subject, a Pole (subject No. 12) had previous familiarity with the names, but even her judgments are in the same direction as those of the other subjects.

3. *Choice of Wearing Smock (F) and not Wearing it (U)*

All subjects wore smocks all during the experiment. On the very last evening, after all had put on their smocks as usual and before work started, the experimenter said casually, "You can take your smock off if you wish. The experiment is all over this evening." One subject (No. 15) stood up and removed her smock. All others kept them on in spite of the fact that it was a hot evening, and there had been scattered complaints at the beginning of the experiment about wearing extra clothes when it was so hot.

At the end of the first hour as E announced the rest period, he said, "The experiment is all over and after the rest period I'll tell you what it was all about. Now you can take your smocks off." Even after this stronger suggestion, subjects

No. 1, 4, 5, 6, 7, 13 and 14 kept their smocks on for the remainder of the evening (53 percent).[2]

Conditions in the control experiment were somewhat different. Here the subjects were asked if they wished to wear smocks to protect their clothes as the room was rather dirty. No one chose to do so (0 percent).

4. *Familiar vs. Unfamiliar Rubber Band*

Rubber bands were used each evening to hold together all the cards on which sentences had been copied. On the 9th evening, an equal number of red (U) bands were mixed in with the familiar gray bands and the piles were put in the customary place. Ten subjects turned in cards with red (U) rubber bands, and five with gray (F) bands (40 percent). E watched the process of choice and it seemed to him that the subjects did not notice any difference and chose at random, reaching into the pile and picking up the first one that came to hand. This was also true in the control experiment (62 percent). In this minor matter, familiarity made no difference at all.

5. *Familiar vs. Unfamiliar Paper Clip*

The most common paper clip available was used as the F clip. A smaller, rounder, less efficient, less easily handled clip was used as U clip. Choice presented in same way as rubber band choice. It was again E's impression that choices were made at random as they picked up the clip which came to hand first. Only five of the fifteen subjects selected the new U clip, the others using the old F clips (66 percent)—Control (62 percent).

6. *Choice of Presence or Absence of Familiar Metronome Click*

On the last evening, the metronome was stopped unostentatiously while the subjects were working. Five minutes later they were asked to stop their work and answer the following questions on a card:

[2] Percentages refer to percent of the group choosing the F (familiarized) alternative.

1. Did you notice it when I turned the metronome off five minutes ago?
2. Which would you now find more distracting, to have it turned on or off?
3. What was the course of the distraction effect during the experiment? That is, did the metronome disturb you more at the end of the experiment than at the beginning, or vice versa?
4. Do you want the metronome turned on now? Yes.... No.....

Several complaints had been uttered in the first two nights about the annoying metronome. In spite of this, 3 subjects answer 'Yes' to question 4, wishing it to be turned on. Only 3 had noticed that the metronome had been stopped. In answer to question 2, nine subjects said it made no difference. Three said it would distract them more if it were turned off. One of these said, "I don't care; rather used to it. Never distracted me much, but had to bear it several days before got used to it and actually liked it somewhat." Another subject said "I liked it after the first few minutes." Except for these comments, the usual picture was one of negative adaptation to an initially slightly annoying stimulus. (No control experiment).

7. *Choice of Different Seat on Last Day*

All were seated alphabetically at the beginning and retained their seats thereafter. Some comments were made on uneven distribution of light at the beginning but when the subjects were told that nothing could be done about it since somebody had to sit at those seats, nothing more was said. On the 9th evening, E made the following statement, interrupting the work being done. "By the way, how many of you would like to change your seat. I had to seat you alphabetically at first, but I know you well now." Nobody answered. (Pause) "Nobody dissatisfied with her seat? Raise you hand if you are. (Pause) Nobody? (looking about at each one). Are you sure? All right, then." Even in response to this progressively stronger suggestion, no one

indicated a wish to change seats, in spite of the grumbling on the first and second evenings. In the control experiment, after all were seated, the above questions were asked. Again no one chose to change her seat.

8. *Choice of Yellow (F) vs. Orange (U) Blotters*

Each subject was supplied with a new yellow blotter each experimental evening until the last. On this evening they were given a choice between the familiarized yellow and an unfamiliar orange blotter. The two colors were chosen to be of about the same brightness.

Two equal piles of yellow and orange blotters were put side by side on a chair near where the books were. Instruction by E "Oh, I forgot to pass out the blotters. Get one yourself as you get your book."

Nine subjects selected an orange (U) blotter (40 percent)—control (46 percent).

9. *Choice of Unlined (F) vs. Lined (U) Cards*

Sentences were copied from the books to these cards, one sentence to a card. All cards supplied had been unlined. On the last experimental day between each pair of subjects were placed two equal piles of cards, one unlined, one lined. These latter were ruled on one side only and this side was turned up. The piles were continually replenished to keep them equal in size.

Eleven subjects chose all F cards (73 percent). Two chose all lined (U) cards. Subject No. 2 chose 10 ruled, 12 unruled and wrote on the unruled side even when she used ruled cards. Subject No. 5 chose 1 ruled, 31 unruled and wrote on the ruled side. In the control experiment, 31 percent chose unruled cards (F), 54 percent chose ruled cards, 16 percent chose half and half ruled and unruled cards.

10. *Familiar vs. New Books to Copy from*

Each subject copied sentences from an individual book, starting from the beginning of the book. On day 8, the following announcement was made as sentences were being copied. "We have a lot of new books if anyone is tired of

this one. Who would like me to bring a new book? (3 subjects raised hands. Repeated their names aloud. Looked about)." "Anybody else?" (Pause). "Sure?" (Pause). "O.K." 80 percent thus chose the F alternative (no control experiment).

11. *Choice of Copying Whole Sentences (F) or Only the Significant Part (U)*

Any sentence that had in it the word in a list supplied them was to be copied. On day 7, E made the following announcement. "I've been told that it isn't necessary to copy out this whole sentence. The only *necessary* thing is to get the word in its context. So if you want to, you can restrict your copying to the *significant* part of the sentence in which the word occurs. By the significant part I mean enough of the context to show exactly what the meaning of the word is in that context, or, if you please, you may continue copying the whole sentence as before. Whichever you do, stick to it. Don't change about."

Eight subjects chose the new (U) way of doing their copying (47 percent F). It should be understood that this experiment was one of several deliberately loaded heavily against the familiar way of doing things. For instance, copying the whole sentence was a very laborious procedure in some cases, since a sentence might be half a page long. I had been asked often if it were necessary to copy out the whole sentence. To the subjects it seemed unnecessary when the only desideratum was to see how a certain word was used. The center of interest becomes then, not the fact that half the subjects abandoned a familiar way of doing a thing, but rather the fact that half the subjects stuck to this way of doing things even when it was less convenient and more laborious than a proffered new way of doing the same job. In the control experiment 85 percent chose the new way, almost twice as many as in the experiment proper.

Similarly it is as interesting for the psychologist that some people refuse a new more efficient, more productive way of farming, or memorizing poetry, or sweeping the carpet simply

because it is new and unfamiliar, as that some people accept it for the same reason or because it is more efficient.

12. *Choice of Copying Sentences (F) or Writing Original Sentences* (U)

On the last day, the following statement was made by E. "You can do as you please today. All rules are off. Someone asked me if it was necessary to copy the sentence from the book. No, it isn't. If you want to and think you'll find it more interesting, you can make up two sentences for each word on your list instead of copying them. If you prefer to copy them, that's O.K. too. Do as you please as long as you get sentences on those cards."

Two subjects chose to do original sentences (U). Thirteen stuck to the familiar way of doing the job (87 percent)—control (92 percent).

13. *Choice of Marking Schemes for True-False Test*

All subjects had been instructed on day 1 to use the following scheme of answering the true-false test.

Write 1 before the statement if you think it is surely true
2 probably true
5 neither false nor true
8 probably false
9 surely false.

On day 7, these instructions were announced casually, as if as an afterthought that didn't matter much. "Some of you said you were bothered by that 1 2 5 8 9 system of answering. It is a little complex and some of you are used to T ? F. If you would like to use T ? F instead of 1 2 5 8 9, go ahead. It won't make any difference in this experiment."

In spite of the fact that U system was more common, less confusing, and more simple, only 7 subjects changed over from 1 2 5 8 9 (F) to T ? F (U); 8 continued to use the F system (53 percent). In the control experiment only 8 percent chose the F system.

On day 9, when given another vocabulary test, these same 7 and 2 more used the U system.

14. *Choice of Yellow Paper (F) vs. Blue Book (U)*

For various tasks for which paper was necessary, yellow paper was available throughout the experiment. On the last day, next to the piles of yellow paper were also piles of blue books (eight pages of ruled white paper bound in blue paper covers). When the time came for the task that required paper to write on, this announcement was made. "Get yourself either a blue-book or some yellow paper and write your name at the top."

Eleven subjects chose the bluebooks (U). Four chose yellow paper (F) (27 percent). In the control experiment 23 percent chose yellow paper.

15. *Choice of Presence (F) or Absence (U) of Wall Paintings*

For this purpose, E had prepared eight tempera color paintings on large sheets of newspring (3′ by 2′). Pure, bright colors were used to produce the most startling and attention-getting paintings possible. No depiction was attempted but instead the paintings consisted of various patterns of color, laid on in definite geometric and curved forms. Attention was further focussed on them by instructions one evening for a few people to get up and make art-criticisms. On day 9 the paintings were removed before the session began and later the subjects were asked to write on a card their answers to these questions.

1. Did you notice that the paintings were missing tonight?
2. Did you find them distracting or annoying at any time during the experiment?
3. Would you prefer to have them hung up again? Yes.... No....

Ten did not notice their absence. No one was distracted by them at any time, eight indicating that they found them 'interesting,' 'bright,' 'cheerful,' etc. Nine voted Yes to the

last question, indicating that they would prefer their presence (F) to their absence (U) (60 percent). No control experiment.

16. *Choice of Familiar (F) vs. Unfamiliar (U) Cooky*

Each session, two cookies were given to each subject. On the last evening they were given a choice of two, one familiar, and one unfamiliar, of roughly about the same size and kind.

The first subject took two as usual, but the others followed the usual polite routine of taking one of each. Only 3 subjects took 2 of the same kind, one taking the new (U) cookies, and two taking the F cookies.

The announcement made was "Well, we've got lots of experimental material left (Laughing). I suppose we might as well finish off the cookies. You can have either kind." 73 percent now chose the F cookies. In the control experiment, 54 percent chose the F cookies.

17. *Choice of Pencil (U) or Pen (F)*

Subjects used fountain pens throughout the experiment, being warned of fines if they used pencil. On day 9, pencils were piled on the table along with the clip, bands, paper, etc. E waited until someone noticed and mentioned the pencils on the table. "Oh, yes! The experiment's about finished now. If you wish you may use pencil or pen. It makes no difference to us. Only if you use pencil, keep on using it; if you use pen, stick to that."

At the moment they were working with mimeographed material for which ink is undesirable, since it tends to run and blot. Eight subjects used pencil (U), seven continuing to use pen (F) in spite of the inconvenience and inefficiency (47 percent).

After 30 minutes of this, they began to copy sentences on the glazed white cards which take ink well. For this all but 4 subjects (73 percent) switched back to pen and ink, in spite of the fact that instructions had been to choose either pen or pencil, and then not change. In the control experiment 0 percent used pen in any of the tasks. All used pencil for cards and papers.

In Table I is represented an attempt to analyze out crudely the individual differences in susceptibility to familiarity among our fifteen subjects. Five scores are considered here; namely, the differences between the F and U scores for the two ratings of the paintings, the differences between the F and U scores for the two ratings of the foreign names, and finally the total of U's scored by each subject in all the other tests. This latter will give us an inverse measure of susceptibility to familiarity, the other four scores being direct measures of this susceptibility. The 15 scores in each column have been placed in rank order in the second part of this table.

Inspection of this table indicates that further and more exact experimentation might show the existence of individual differences in susceptibility to familiarity.

III. Discussion

No simple formula is available for explaining the results we have obtained. We have already indicated above that at least two factors would have to be considered in such an explanation, namely increased liking as a result of familiarity, and decreased liking as a result of familiarity. This latter may possibly (not certainly) be expressed as a liking for novelty.

It was the experimenter's general impression that in certain of the choice tests, familiarity made no difference at all in either direction simply because the choices were of no importance at all for the subject (paper clips, blotters, rubber bands, etc.).

It was impossible to control adequately the factor of previous familiarity with the two members of each choice, *e.g.*, pencil or pen, yellow paper or white paper. The F paper clip is far more common than the U clip, the gray rubber band is far more common than the red.

The writer is inclined to lay most stress on the results of the first two tests (paintings, foreign names) since these tests were treated most adequately and the data are most easily treated statistically. All differences are in favor of familiarity. The differences in favor of familiarity are statistically significant or approach significance. Also the factor of

previous familiarity is probably best controlled in these two experiments.

In general, also, the other data seem in the writer's opinion to point to a greater familiarity effect than might ordinarily be suspected. Some of the experiments indicate further that the tendency to prefer the familiar might theoretically be equated with some such concept as 'psychic inertia' which is not the same thing as laziness or physical inertia since the latter is often overbalanced by the former, *e.g.*, some of the tests showed that a longer or more laborious procedure may be preferred simply because it is familiar.

Another necessary, but difficult, task is to disentangle theoretically the effects of ordinary 'learning' from affective change in these experiments. Granted a very broad concept of learning in which the whole situation is an integral portion of the learning process, we might, by stretching a point here and there, say, *e.g.*, that an unruled card (F) rather than a ruled card (U) was chosen by most of the subjects simply because the stimulus of the whole situation (experimental setup, particular room, particular kind of work, particular sets and expectations) 'called for' the response of taking the unruled card (F), not because there was affective change, or because the subjects 'preferred' them. In the same way, a learned bond might have been formed between the whole complex situation and a particular scheme of answering the questions. It is conceivable that these same subjects, on the same day, but in a different milieu which 'called for' different responses, might have 'preferred' ruled to unruled cards, pencils to pens, etc.

It seems to us that such an interpretation cannot, given any amount of point-stretching or leniency of concepts, be applied to the paintings test and to the foreign names test. In these there must have been true increase in preference. It seems to us that the data permit of no other interpretation. In these tests there was no mere choice involved that might or might not be 'called for' by the situation. There was actual rating on the frank basis of like and dislike. This interpretation is reinforced by the fact that, even where like

and dislike were not frankly involved (as in the ratings for beauty and euphony) familiarity nevertheless played an important role in determining the direction of these supposedly objective ratings, since it is possible to assume that such 'objective' ratings are nevertheless measures of affective preference.

(Manuscript received January 16, 1937)

A TECHNIQUE FOR THE KYMOGRAPHIC REGISTRATION OF CERTAIN ASSOCIATED VOLUNTARY MOVEMENTS

BY REX MADISON COLLIER*

Northwestern University

Researches upon movement have, in true scientific procedure, usually attacked the simpler relationships considered to be basic to all movement. Thus, muscular contractions have been recorded in excised nerve-muscle preparations; the behavior of reflexes in spinal, decerebrate and decorticate animals has been studied; characteristics of excitation, inhibition, reciprocal innervation, synergic co-contraction and co-relaxation have been investigated. All these relationships, as well as physico-chemical conditions, are basic to movement and are most complexly organized in voluntary movement. The study of voluntary activity may be directed toward one or more of these basic relationships just indicated or toward the final pattern of movement itself. However, activity initiated by or dependent upon cortical and pyramidal tract control presents a peculiarly complex problem. The problem is complicated not only by the vast interrelationships of sub-cortical mechanisms, but also by relatively unstable conditions of cortical function designated psychologically as attitudes, mental sets and determining tendencies. Methods for the registration of types of voluntary movement are of importance in promoting investigations of those relationships which subserve the final movement patterns. Among those factors known to modify the form of voluntary movement are the mental set from which movement is organized, practice, fatigue, effort, concomitant tasks, restrictions both anatomical and artificial which limit the movement, and disorders of the neural centers of motor

* From the Department of Nervous and Mental Diseases, Northwestern University Medical School.

control. The present study is concerned, in part, with modifications of a voluntary movement pattern which are associated with clinical types of neurological disorders.

The specific purposes of the report are (1) to describe an apparatus designed to register kymographically voluntary pronation-supination movement patterns, (2) to present a series of typical records, and (3) to indicate what appear to be fruitful lines of research. The apparatus shown in Fig. 1 was developed as a part of a larger project, the purpose of which has been to devise methods of recording qualitatively and quantitatively certain disorders of movement recognized as symptomatic in clinical diagnoses (3)

Apparatus

The essential details of the apparatus are illustrated in Fig. 1. The rotary shafts are made of half inch brass rods which are mounted in sturdy one-half inch bearings. A handle with a three quarter inch grip is braised to the end of each shaft. To the forearm supports are attached heavy sponge rubber pads one and one-half inches thick. A rounded trough was hollowed out along the center of the rubber pads in order to give a firm, but slightly pliable, support to the subject's forearm. This type of support has proved to be very successful in maintaining the forearm in a constant position, while at the same time being sufficiently flexible so as not to impede rotary movements. At the opposite end of the shaft is wound a waxed thread. This thread is conducted to the record marker by glass tube bearings. The glass tubing is a nearly frictionless contact for a waxed thread, and very little resistence is offered to a rapid to and fro movement. The record marker was constructed so as to move in a vertical straight line direction. This was accomplished by using a 1/16th inch shaft of drill rod to which was soldered the spring brass pointer. The brass bearing through which the drill rod was passed was held in position by a ringstand clamp. This bearing was 1½ inches long. To prevent lateral movements of the marker the hole through the center of the bearing was only 1½ thousandths of an inch larger than the drill rod. Two of these markers were used, one for the right arm records, the other for the left. The special advantages of this type of record marker will be discussed later.

During rapid rotary movements, the pointers as described above, due to their momentum on the upward progression were found to overshoot slightly the extreme rotation of the arm in that direction. This slight artifact in the pattern was corrected by attaching a light spring to the base of the marker shaft. The most convenient type of spring was found to be a thin strip of spring brass one inch wide and about twelve inches long. One end is fastened to the table top, while the other end is attached with a thread to the lower end of the marker shaft.

The time marker is a Joseph Becker instrument. The writing lever is capable of as many as 120 complete oscillations a second. This rate may be obtained by the usual 60 cycle current with transformer in the circuit to deliver a six to eight volt current. For slower speeds the marker may be activated by pendulum, metronome, electric fork or other vibrators. The vibrating reed device sold by the Harvard Apparatus Company has been found very satisfactory. The steel reed in our apparatus

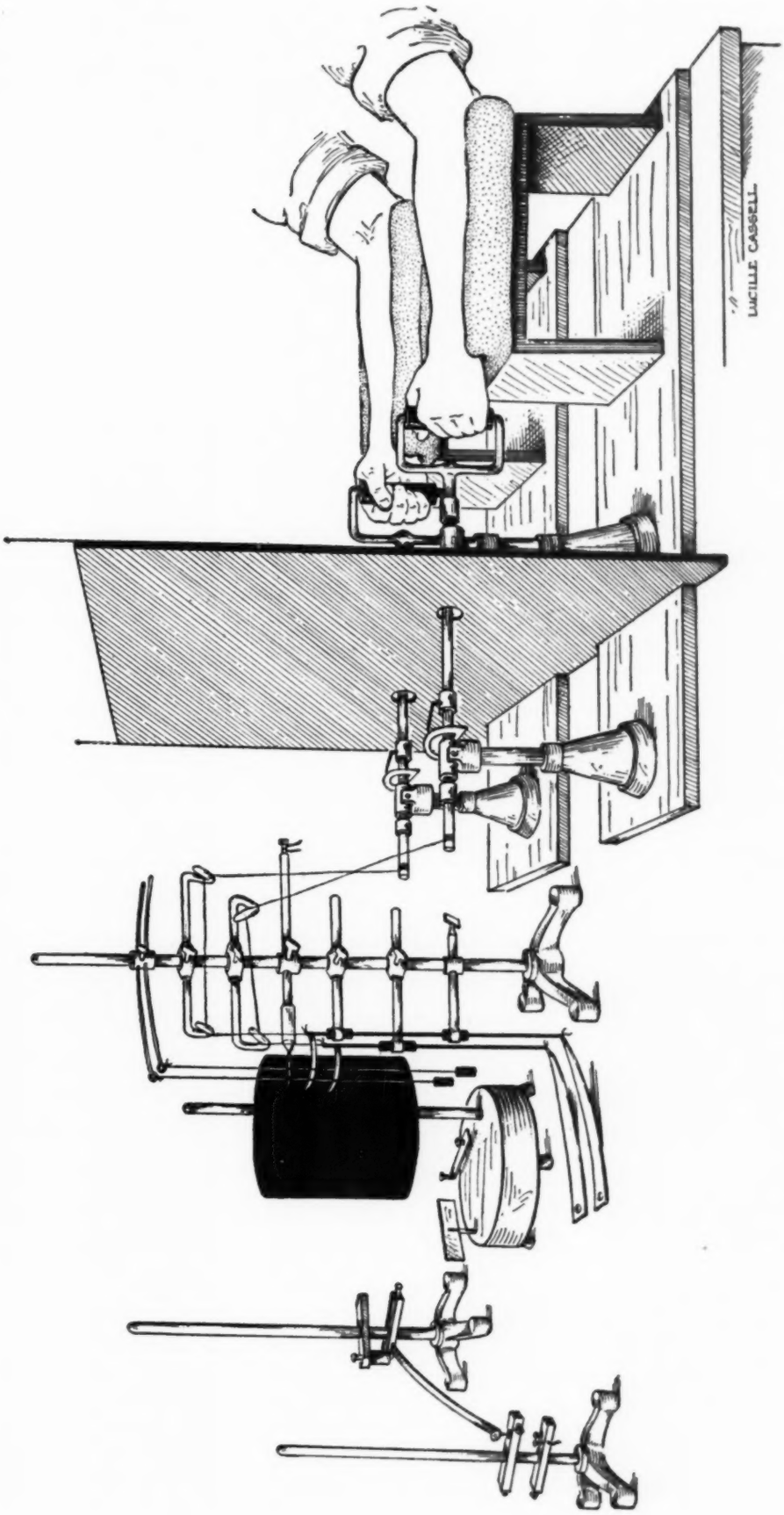


FIG. 1

has been calibrated to oscillate at three, five, ten and twenty times a second. A double pole, double throw switch in the circuit permits the marker to be connected either to the city alternating current or to the mechanically interrupted D.C. battery current.

By means of a series of steps, the height of each subject can be adjusted so that the angle between forearm and upper arm is approximately 100 degrees.

A screen between the subject and the kymograph protects him from the distraction of observing his own record while performing the required task.

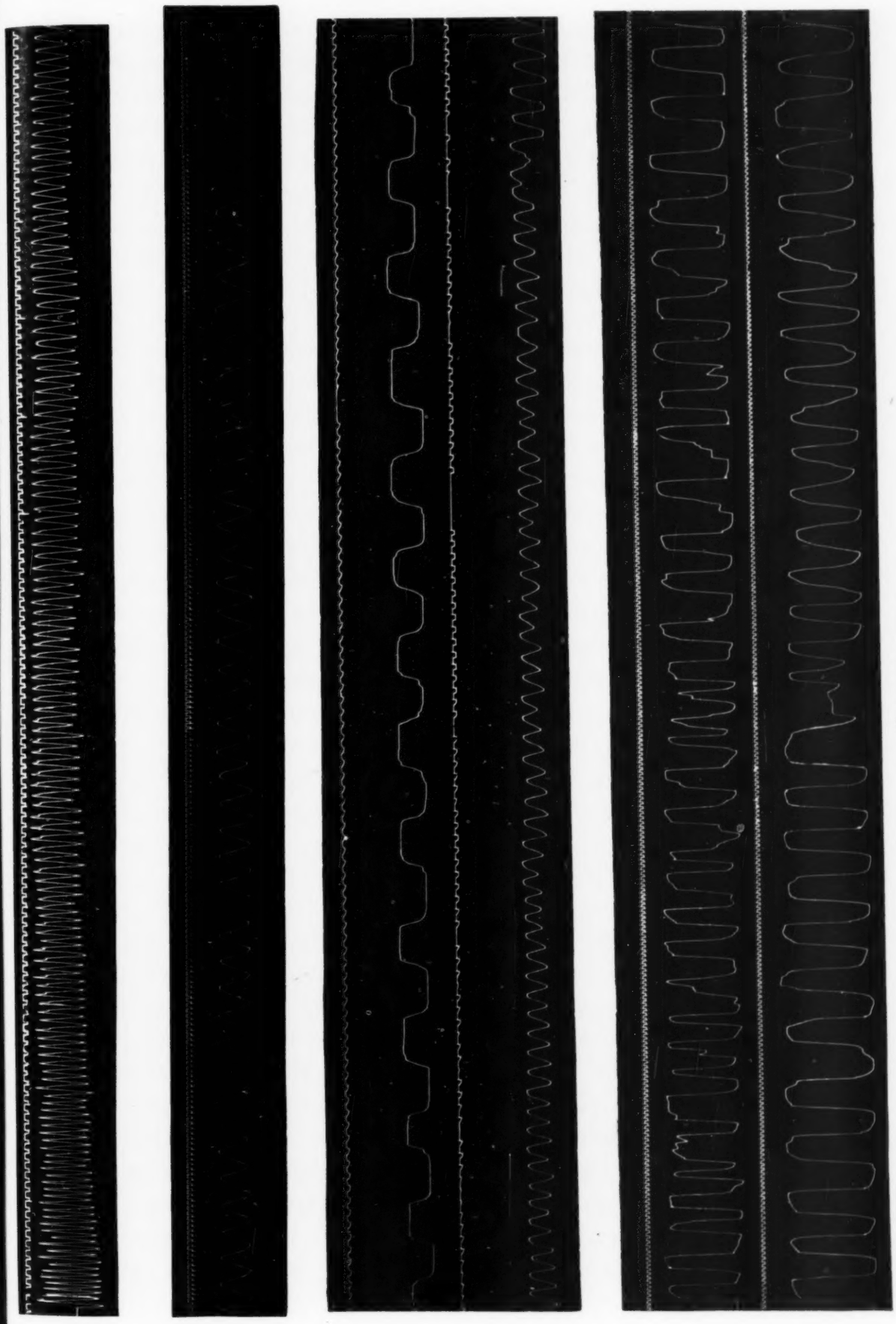
Instructions.—Instructions to the subject depend upon the type of experiment to be performed. They have, for the accompanying records, consisted in showing the subject or patient his position in relation to the apparatus, how to grip and to move the handles. He was instructed to hold the handles with just sufficient tension to maintain a firm grasp. When the experimenter gave the signal 'go,' he was to rotate the designated hand (*i.e.*, right, left, or both) as far and as fast as possible. A few short preliminary trials must always be given to determine if he is coöperating correctly.

Typical Records

Since this report purposes only to indicate what the apparatus herein described can show, the accompanying records must be taken as illustrative and not as conclusive in regard to the disorders mentioned. Furthermore, no attempt is made at this time to analyze completely each set of records. That task is reserved to more extended research upon each type of case. It is sufficient here to indicate the variety of movement patterns that these cases exhibit and to suggest the lines of research that are being developed.

The rate of drum movement was practically the same for all of the records. The time line interval of record 1 is ⅓ of a second. For all the rest the interval is $^1/_5$ second.

Record 1.—This represents a fairly typical normal right arm record. The amplitude of the initial rotations is about 158 degrees, while that of the last few rotations is about 100 degrees. The total work period shown here is approximately forty-two seconds. During the first ten seconds there were forty complete movements (average of thirty normal cases found to be thirty-two complete movements), while in the last ten seconds, there were twenty-three and one-half. Were this type of movement to be used in a fatigue study, work decrements should be expressed in terms of degrees of arc through which the forearm moves in a given period of time. Neither rate nor amplitude by itself is an adequate measure. Attention is called to the pattern form near the



RECORDS 1 TO 4

middle of the record. At the end of the supination movement, or the upward direction of the marker, there appears a sharp peak with a shoulder below. Other records obtained by a more rapid rotation of the drum have indicated that the shoulder effect is probably due to a delay in the contraction of antagonists which produce the movement in the opposite direction. The relationship of the patterns of co-contraction and co-relaxation of antagonists in rapid voluntary oscillatory movements merits further study (**1**, **4**, **12**).

Record 2.—This record was obtained from the right arm of a woman, age 34, who was diagnosed as presenting a paramyotonia acquisita. The myotonia was atypical in that repetition of movement appeared to increase muscular tension with accompanying retardation instead of rendering a progressive facility to the successive movement which is characteristic of Thomsen's disease. This retardation of movement may be observed in the latter part of the record. The record indicates a slow contraction of muscles in both directions. The nature of the reversal at extreme pronation (downward movement) indicates an accompanying slowness of relaxation of the muscles just completing the movement. The average amplitude of the rotation is 77 degrees, while only ten complete movements are made in a ten second interval. The left arm gave the same type of pattern; it is not reproduced here.

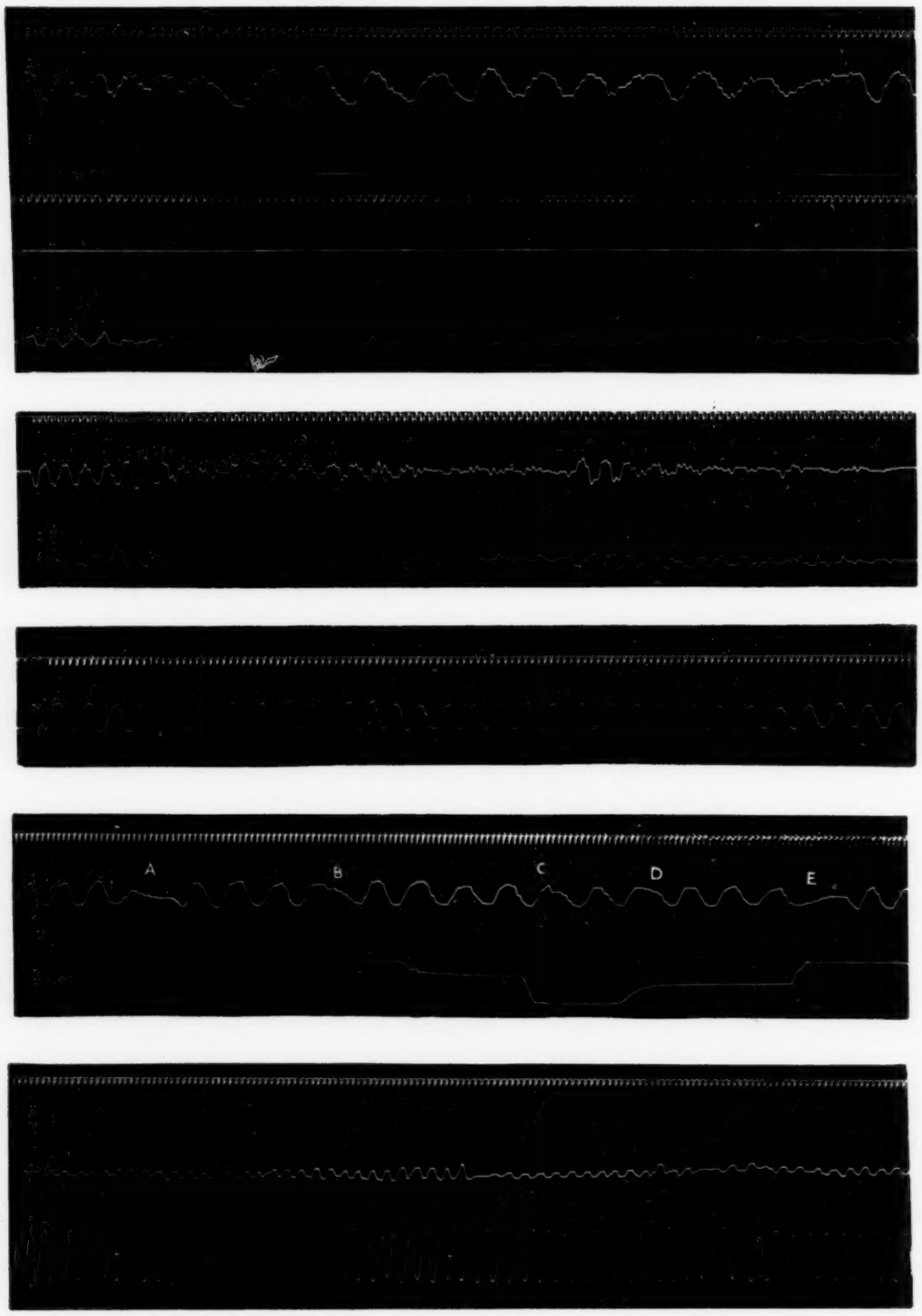
Record 3.—These are tracings of both right and left arm movements of a man, age 46, with a right hemiplegia of luetic origin. The upper record is for the right hand; the lower for the left hand. The exceedingly long period required for the reversal of the movement is the most evident feature of the right arm tracing. That this long reversal time is not due primarily to a tonic state of the muscle is indicated by the fact that the progression between the extremes of rotation is almost normally rapid. The rate of total movements is principally reduced by the abnormal extension of reversal times which are about 30 times normal length. The amplitude of rotation of each arm is only about

63 degrees. The rate of the right arm is 6½ complete turns in ten seconds; the rate of the left arm is 31½ such turns in the same length of time. Records such as these permit a study of some of the conditions which modify the components of a voluntary movement (**2**).

Record 4.—The patient (male, age 37) from whom this tracing was obtained was diagnosed as presenting multiple sclerosis. Separate tracings were made of the voluntary movement patterns of both right (upper) and left (lower) arms. The average amplitude of the right arm movement is 185 degrees, while that of the left is 220 degrees. Right arm rate is about eight complete turns in ten seconds; only five are made with the left in a similar time interval. The general pattern of the tracing is of interest. Both adiadochokinesis and hypermetria appear to be present. Attempts to reverse the direction of the movement evidence not only delay in the initiation of the movement, but occasionally what appear to be rival impulses in competition. Delay in the reversal of the direction of movement is probably associated also with the abnormal extent of the rotation. These patterns, as well as those of records 2 and 3, have characteristic differences from the normal and from each other. The tracings made by Tilney and Pike (**12**) on antagonistic muscular contractions in cases of multiple sclerosis, Huntington's chorea, etc. portray antagonistic muscular relations basic to the production of final movement patterns, but they do not show what those final movements actually were. Simultaneous records of the final voluntary movement with the behavior of antagonistic muscles subserving the movement would be of value in more complete descriptions of normal and pathological reactions (**1**).

Records 5 and 6.—These two records were obtained from a patient (male, age 41) who had developed Parkinsonian symptoms following an attack of encephalitis lethargica. Record 5 shows at the top the right arm tracing, below the left arm tracing. The bradykinesia, characteristic of these cases, is evident. Tremor effects may be noted as super-

imposed upon the voluntary movement. The maximum amplitude of the right arm rotation is 92 degrees, while the maximum rotation of the left is 36 degrees. Muscular rigidity was so pronounced on the left side that very little voluntary movement was possible. Record 6 shows the



RECORDS 5 TO 9

crossed effects of simultaneous bilateral movement found in some of these cases. Comparison of the two sets of tracings reveals that the right, or less affected side, tends to take on the characteristics of the contralateral side when both are given simultaneous voluntary innervation. Crossed innervation accompanying voluntary effort has been observed by Wilson (**16**). He notes that "abduction of the little finger on the weak side is accompanied by the same movement of the normal finger, extension of the wrist on the affected side by extension of the wrist on the other, and so on" (p. 179). Crossed effects more similar to those mentioned by Wilson may be seen in the next two records.

Records 7 *and* 8.—These tracings are from a patient (male, age 37) who had developed the classical picture of the Parkinsonian syndrome. Record 7 is from the right arm. Slowness of movement, reduction of range and slight tremor may be noted. In this case it was easy to elicit a tremor by a quick, passive rotation of the forearm.[1] Record 8 is exhibited to show what appears to be crossed effects of contralaterally induced tremor. The patient was instructed to grip both handles of the apparatus, but to move only the right one (upper). During the movement, the experimenter made quick passive rotations of the left arm. The record of these passive movements is shown by the lower tracing. At points *a*, *b*, *c*, *d*, and *e* the contralateral effects of this induced tremor are expressed. The tremor on the ipsilateral side is not so evident in the tracing because the experimenter's grip on the patient's hand tended to reduce its amplitude. Partial inhibition of rate and amplitude of the contralateral voluntray movement together with increased tremor are exhibited at the designated points of the tracing. In Record 7 a slight tremor accompanying the voluntary movement may be noted. Modification of right arm movement in Record 8 may be a facilitation of tremor already present rather than the introduction of a new series of responses. In either case the neural

[1] It is of interest to compare the records of this patient with those exhibited by Wilson (*ibid.* p. 197, Fig. 22) as portraying the 'cog-wheel' phenomenon. Turner and Critchley (**13**), who studied breathing records in parkinsonian patients, note a similar pattern of successively interrupted movements.

centers essential to these associated responses become of particular interest in relation to other problems concerned with the integration of bilateral movements.

Record 9.—This tracing is from a normal individual. It is included to show the extent to which involuntary associated movements may develop simultaneously with a voluntary movement. The individual was right handed. For this record he was instructed to grip both handles but to move only the left. Numerous other instances indicate that these involuntary associated movements are more likely to occur on the habitually preferred side when the non-preferred side is being voluntarily operated. The marked individual differences in the degree to which voluntary movements may be dissociated laterally (**6**) is a problem to which attention may be later given.

Special Advantages of the Apparatus

The straight vertical movement of the record markers permits not only a more direct translation of the movement pattern to the kymograph drum but also allows a constant alignment of the markers. This alignment can be checked at the beginning of each record by moving the pointers up and down to see if they follow in the same path. Where the markers operate from a pivot, corrections must be made for the arc through which the pointer moves; but what is more serious in taking simultaneous records is that there is no vertical alignment of the pointers except when both are making the same angle with reference to the line joining the centers of the circles which the markers describe. This moment may only rarely occur in actual record taking. Study of simultaneous and nearly simultaneous events is paramount where problems of functional integration are considered.

If speed of voluntary movement is to be studied in relation to special conditions, the apparatus described here should prove superior to the usual tapping devices. The early single tapping plate (**17**) was discarded because the subject could alter his score significantly by developing a sort of voluntary

tremor. The double tapping plate (**8**) was introduced in order to avoid this source of error. Here the subject moved the stylus back and forth from one plate to another. Recently (**2**), the tapping frame has been revived. The subject is required to oscillate his fore finger, or a tool, between upper and lower limits of the frame. With all these devices the only records possible are quantitative. Furthermore, the amplitude of the movement is either inadequately controlled or the mechanical limitations imposed upon the movement pattern in order to control it prevents a study of the qualitative reversal characteristics at the extremes. The pronation-supination movement of the forearm can be expressed both qualitatively and quantitatively by means of the present apparatus, while at the same time the structural characteristics of the forearm impose their own natural limitation upon the movement. Character and frequency of movements here may be studied not only under arbitrary mechanical limitations, but may be compared with those when only anatomical limitations are present. Data already collected indicate significant differences particularly in relation to the components of the movement.

The tapping movement has frequently been studied in connection with fatigue experiments (**2**, **15**, **17**). But since the muscles of the whole arm are brought into a response where any one of several groups of these muscles may produce a similar movement, the results are complicated by the fact that muscle groups may be alternated in the responsibility of effecting a similar result. True effects of fatigue are masked since it becomes possible for muscle groups to rest and to resume activity without the fact being expressed in the records. In a rapid pronation-supination movement where the forearm is held at a ninety to one hundred degree angle with the upper arm, movement is confined throughout the series to the same groups of antagonistic muscles. Very little, if any, alternation of muscle groups is possible. It is probably because of this that decrements in rate and amplitude ordinarily occur rather early in the movement in comparison to the tapping experiments. Attempts, only partially

successful, have been made to study the development of muscular tension and increased outputs of energy in the tapping stroke with prolonged work periods (**5**, **7**, **9**, **10**, **14**). A simple modification of the apparatus described here has made possible a study of a similar associated effect. The solid rotary shaft is replaced by a tube with outside diameter of the same dimension. The bore has a diameter of 1/16 inch. In place of the handle shown in Fig. 1, a dynamometric device is used. A thread from the moveable lever of the dynamometer is passed through the center of the tube and is attached to a writing lever similar to those illustrated in the drawing. Changes in grip tension during an experiment are directly translated to the kymograph record.

Other problems which can be studied by this apparatus or modifications of it are (1) the components of the pronation-supination movement (*i.e.*, progressions and reversals) and the conditions which modify them; (2) motor leads in discrete and continuous series of simultaneous bilateral movements and conditions which modify those leads; (3) the mutual modifications of concomitant tasks; (4) the mutual effects of bilateral associated movements with reference to locus of lesion in neurological disorders. Principally through studies of the mutual modification of associated movements in relation to the conditions of those modifications has research been able to discover facilitative and inhibitory characteristics of integration.

Summary

An apparatus has been described which permits graphic registration of voluntary movement patterns expressed through forearm rotation. Typical movement patterns from normal and neuropathological cases have been illustrated and described. These records exhibit characteristic differences between the normal and pathological cases and among the pathological cases themselves. Bilateral modifications of voluntary movement and contralateral effects of a passively induced tremor are illustrated in cases of parkinsonism. Special advantages of the technique are pointed out for studying conditions associated with motility rates, fatigue,

forearm muscular tension, and, in particular, bilaterally paired movements.

(Manuscript received January 11, 1937)

References

1. Altenburger, H., Physiology and pathophysiology of coördination; adiadochokinesis, *Zeits. f. d. ges. Neurol. & Psychiat.*, 1932, **142**, 373–412.
2. Boder, D. P., The influence of concomitant activity and fatigue upon forms of reciprocal hand movement and its fundamental components, *Comp. Psychol. Monog.*, 1935, **11**, No. 4, 1–121.
3. Collier, R. M., A pursuit device for obtaining both quantitative and qualitative records, *J. Psychol.*, 1936, **2**, 295–300.
4. Dodge, R. and Bott, E. A., Antagonistic muscle action in voluntary flexion and extension, *Psychol. Rev.*, 1927, **34**, 241–272.
5. Dorcus, R. M., Performances of athletes in coördination tests, *J. Comp. Psychol.*, 1923, **3**, 475–499.
6. Drinkwater, H., The left-handed child, *Brit. Med. J.*, 1924, **1**, 1113.
7. Duffy, E., Tension and emotional factors in reactions, *Genet. Psychol. Monog.*, 1930, **7**, 1–79.
8. Dunlap, K., Improved forms of steadiness tester and tapping plate, *J. Exper. Psychol.*, 1921, **4**, 430–433.
9. Johnson, B., Changes in muscular tension in coördinated hand movements, *J. Exper. Psychol.*, 1928, **11**, 329–341.
10. ——, *Child Psychology*, Baltimore: Thomas, 1932. Pp. 183f.
11. Lebedinsky, M. S. and Luria, A. R., Die Methode der abbildenden Motorik in der Untersuchung der Nervenkranken, *Arch. f. Psychiat. u. Nervenkrank*, 1929, **87**, 471–497.
12. Tilney, F. and Pike, F. H., Muscular coördination experimentally studied in its relation to the cerebellum, *Arch. Neurol. & Psychiat.*, 1925, **13**, 289–334.
13. Turner, W. A. and Critchley, M., Respiratory disorders in epidemic encephalitis, *Brain*, 1925, **48**, 72–104.
14. Wellman, B. L., The development of motor coördination in young children: an experimental study in the control of hand and arm movements, *Univ. Iowa Stud. Child Welfare*, 1926, **3**.
15. Wells, F. L., A neglected measure of fatigue, *Amer. J. Psychol.*, 1908, **19**, 345f.
16. Wilson, S. A. K., *Modern Problems in Neurology*, New York: Wood, 1929.
17. Whipple, G. M., *Manual of Mental and Physical Tests*, Baltimore: Warwick and York, 1914, Vol. 1.

COMPETITIVE BEHAVIOR IN THE ALBINO RAT

BY WILLIAM M. LEPLEY

The Pennsylvania State College

Introduction

The simple experiment to be reported was performed in an attempt to answer the question: can rats be induced to exhibit behavior analogous to that which is loosely called competitive with reference to human behavior?

The most casual observation of a group of rats at feeding time reveals that the animals forcefully oppose one another in the press about the food receptacle. Harlow (**2**) has described this behavior as competitive. In this study, Harlow concludes that: "The essential condition for the occurrence of social facilitation is the presence of rats unrestrained and actively competing with each other for food." He demonstrated that the food consumption was greater in these competitive circumstances. Several animal studies have shown this phenomenon of social facilitation and that it is intimately related to the sort of behavior ordinarily called competitive. Harlow's study of monkeys (**3**) provides a further example. In this case, competition, although probably the most important factor, does not seem to be essential to the appearance of social facilitation. This seems to be also true in Bayer's study of the feeding of hens (**1**).

In the experiment reported below, an attempt was made to arrange the competitive circumstances so as to require of the animal: (1) a discrimination between a pre-goal winning situation and a pre-goal losing situation; and (2) sustained, appropriate anticipatory activity in order to attain the goal for which it is competing.

The Experiment

Apparatus.—The apparatus consisted of a straight unobstructed alley six inches wide, five inches high and thirty feet long (inside dimensions). This alley was constructed of wood, painted a dull black and provided with a top of wire mesh. The

starting box was divided into two compartments by a longitudinal partition and was closed at the forward end by a vertically sliding gate. This gate was so wired that the opening thereof closed the circuit through a magnetic signal marker arranged to record upon a constant speed polygraph (paper speed = 7.86 inches per minute). The goal box was also divided into two compartments and was closed by a fixed gate over which the animals were required to climb or jump. This goal box was situated six inches beyond an arbitrarily established 'finish line,' which was plainly marked upon the floor of the alley. The distance between starting gate and 'finish line' was exactly thirty feet. An animal's run was recorded as complete when any portion of the body crossed the 'finish line.' The time of crossing was recorded on the polygraph by a magnetic signal marker which was manually regulated by the experimenter. The apparatus as a whole was operated behind a screen placed at the goal end of the alley. Running time was obtained by reading the polygraph record with an appropriately graduated scale.

Subjects.—The experimental animals used were albino rats. The group consisted of twelve litter mates, six males and six females. These rats were one hundred and twenty-five days old on the first experimental day.

General Procedure.—1. Throughout the experiment the animals were run after a starvation period of approximately forty-seven hours. The food reward consisted of pulverized, one hundred percent whole wheat bread mixed with whole milk. Each animal was given four trials per day.

2. After each day's runs the animals were fed to satiation upon this same diet.

3. All experimentation was scheduled between the hours of 5:30 and 6:30 p.m., at which time extraneous noise was at a minimum.

4. The ample, artificial lighting of the apparatus was held constant.

5. Throughout the experiment the positions of the animals in the double starting box were varied according to a chance order.

Specific Procedure.—The successive experimental procedures were as follows:

1. Each animal was given twenty, manually guided (if necessary), untimed, rewarded runs in isolation.

2. Each animal was given eighty unguided, untimed, rewarded runs in isolation.

3. Each animal was given forty unguided, timed, rewarded runs in isolation. The mean time scores for these ten successive experimental days are presented in Table I and represented graphically in figure 1. Each point in this phase of the graph is based upon the mean of forty-eight measurements.

4. Upon the basis of the mean time scores obtained in the foregoing procedure the animals were then matched and paired within each sex group. The mean score for each animal was based upon forty measurements. The range of these mean scores was from 14.2 seconds to 8.5 seconds. These pairs were then given forty unguided, timed, rewarded runs *in pairs*. In this procedure and in the later procedures one animal was placed in each compartment of the starting box and the order of placement was varied according to chance. Food was placed in each of the two goal compartments. The mean time scores for these ten days appear in Table II and are represented as days 11 to 20 in the graph. Here again each point represents the mean of forty-eight measurements. Though the animals ran in pairs they were timed independently. In this procedure the order of crossing the finish line within each pair varied inconsistently from trial to trial which indicated that the pairs were well matched.

5. These pairs were then given one hundred and sixty additional unguided, timed runs. In these one hundred and sixty runs only the winning rat in each run, that is to say, only the animal first to cross the finish line, was rewarded. This was accomplished

TABLE I[1]

THE SPEED OF LOCOMOTION OF RATS RUNNING IN ISOLATION
Each value in this table represents the mean of four measurements
Time in Seconds Tabulated by Days

Animal	1	2	3	4	5	6	7	8	9	10
Male 1	14.1	13.8	14.1	15.6	12.8	11.0	15.6	14.7	18.4	11.9
Male 2	16.2	10.0	8.4	8.8	11.1	8.4	10.5	10.5	9.7	9.7
Male 3	9.5	8.2	8.7	14.0	10.7	9.6	9.2	8.9	7.7	9.7
Male 4	11.0	10.2	10.0	10.7	8.6	12.0	10.6	10.4	15.2	12.2
Male 5	10.7	11.9	12.6	11.5	12.6	14.8	14.4	13.0	11.2	10.2
Male 6	10.0	12.0	7.5	11.8	8.8	8.6	8.2	11.1	9.0	9.7
Female 1	8.0	7.4	7.4	8.9	6.9	10.7	10.0	8.2	9.4	7.5
Female 2	7.4	10.4	7.3	9.5	8.0	8.7	8.3	10.3	11.9	8.5
Female 3	9.5	8.3	9.7	10.2	8.6	8.2	9.3	8.8	6.7	7.1
Female 4	9.9	13.3	8.9	11.5	8.8	9.9	8.6	10.1	9.4	11.7
Female 5	12.0	9.0	10.1	8.3	7.5	7.8	8.0	11.0	9.7	9.1
Female 6	10.6	11.1	10.0	8.1	7.3	12.4	8.9	8.4	8.3	7.2
Mean	10.7	10.5	9.6	10.7	9.3	10.2	10.1	10.5	10.6	9.5

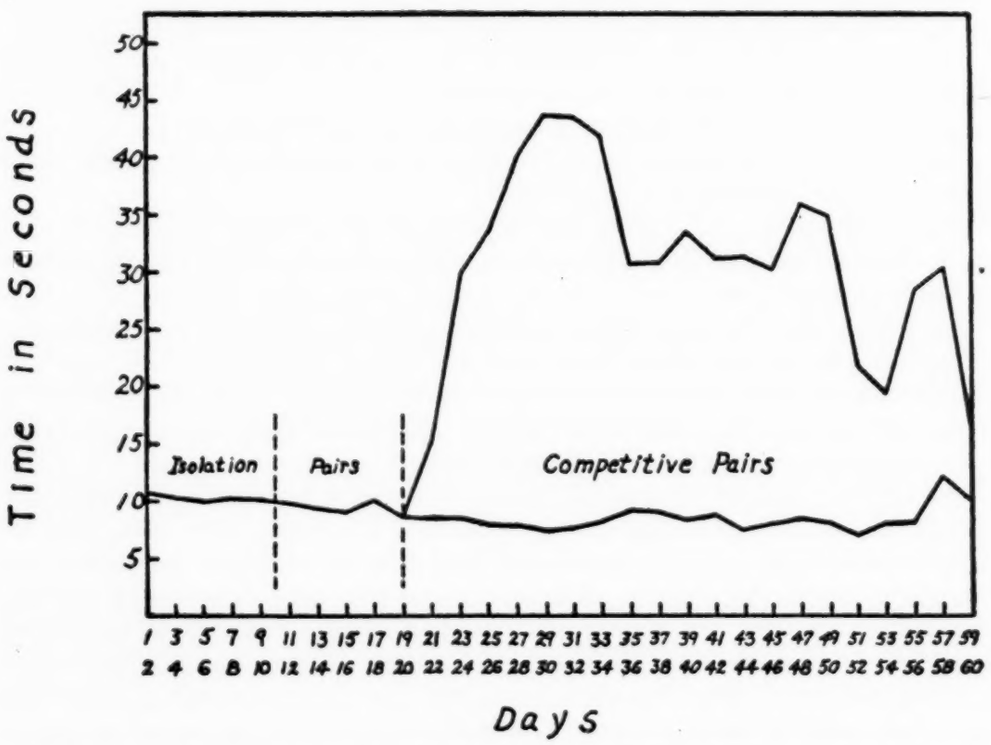


FIG. 1. This graph represents the data in Tables I, II and III. Each point on the graph from day 1 to day 20 inclusive represents the mean of forty-eight measurements. Each point on the graphs from day 21 to day 60 inclusive represents the mean of twenty-four measurements. The upper line in this phase represents the performance of the losers. The lower line represents the performance of the winners.

[1] The complete records from which the summary tables in this study were compiled are on file at the Department of Psychology of The Pennsylvania State College.

TABLE II

THE SPEED OF LOCOMOTION OF RATS RUNNING IN PAIRS
Each value in this table represents the mean of four measurements
Time in Seconds Tabulated by Days

Animal	11	12	13	14	15	16	17	18	19	20
Male 1	11.9	13.1	12.4	10.6	11.1	11.2	12.5	10.8	9.7	9.5
Male 2	10.7	12.7	9.9	8.9	10.9	9.0	9.6	12.0	8.6	11.9
Male 3	9.6	8.6	8.7	8.9	7.4	7.2	9.4	9.5	9.6	9.4
Male 4	11.4	8.3	10.1	8.8	8.7	9.0	10.3	12.0	8.9	10.6
Male 5	11.1	13.9	13.4	10.3	11.0	9.7	8.5	10.1	8.1	8.0
Male 6	8.8	7.6	7.8	8.1	6.8	6.9	9.1	9.7	8.9	8.8
Female 1	11.2	7.4	9.3	6.6	7.5	9.3	7.4	8.0	6.9	7.9
Female 2	7.6	8.2	9.5	9.2	8.0	8.3	7.8	9.2	6.9	7.0
Female 3	8.6	7.7	11.5	8.4	8.1	7.4	7.6	9.8	7.8	8.8
Female 4	11.0	12.0	8.9	9.6	9.2	11.5	12.7	12.8	8.2	9.7
Female 5	8.9	10.1	7.7	8.1	10.0	10.5	12.5	10.8	8.6	8.9
Female 6	7.1	8.1	9.8	9.5	7.6	8.6	9.0	11.0	7.8	6.8
Mean	9.8	9.8	9.9	8.9	8.9	9.1	9.7	10.5	8.3	8.9

by placing food in only one of the goal-box compartments and by requiring the losing rat to enter the unbaited side. In the early stages of this procedure it became necessary to establish a criterion of refusal to run because the losing animals rapidly slowed down and occasionally refused to leave the starting box. A run was scored as a refusal if at the end of sixty seconds the animal had not crossed the finish line. These scores were included in the computations. The mean time scores obtained by this procedure appear in Table III and are represented on the graph as days 21 to 60. In this phase of the graph the scores of the winners and the scores of the losers are plotted separately. The upper line represents the losers and the lower line the winners. Each point in each graph represents the mean of twenty-four measurements. *This method of plotting is made legitimate by the fact that from the twenty-third day to the fifty-eighth day inclusive the personnel of the losing group was always the same.* Obviously then the personnel of the winning group was likewise constant. In other words, six of the twelve contestants ran for thirty-six days or one hundred and forty-four trials without food reward. On the fifty-ninth day one female loser began winning. On the sixtieth day a second loser, also a female, began winning. At this point the experiment was discontinued.

RESULTS AND INTERPRETATIONS

1. The results from the last ten days of runs in isolation appear to show that the group had been trained to a speed of locomotion plateau. A comparison of the mean scores for the first five of these days with the mean scores for the last five days reveals an obtained difference of 0.0.

2. The data from the first ten days of trials in pairs, in which both winner and loser were rewarded, appear to show

that the interstimulation resulting from running in pairs was adequate to produce an increase in the speed of locomotion. A comparison of the mean scores for these ten days with the mean scores for the preceding ten days of isolated trials reveals a difference of 0.8 second which is 3.08 times as large as the standard error of the difference. This interpretation

TABLE III

THE SPEED OF LOCOMOTION OF RATS RUNNING IN 'COMPETITIVE' PAIRS
Each value in this table represents the mean of four measurements
Time in Seconds Tabulated by Days

Animal	21	22	23	24	25	26	27	28	29	30
Male Winner 2	10.2	8.9	7.9	10.0	9.3	6.7	7.2	7.0	6.3	8.5
Male Winner 5	9.4	7.3	6.9	6.5	8.5	7.2	8.3	7.6	6.9	7.2
Male Winner 6	9.2	9.4	9.8	8.6	7.8	10.3	8.9	7.3	6.7	7.4
Female Winner 5	8.9	8.7	7.5	9.9	8.6	8.8	9.7	11.0	9.5	9.3
Female Winner 2	6.4	7.4	6.8	7.1	7.6	7.4	7.5	6.8	6.4	7.2
Female Winner 1	7.9	7.3	8.8	11.3	5.9	6.6	6.5	6.2	6.1	6.9
Mean	8.7	8.2	8.0	8.9	8.0	7.8	8.0	7.7	7.0	7.8
Male Loser 4	10.4	11.8	32.0	31.3	34.6	32.0	45.8	48.7	49.4	50.3
Male Loser 1	9.5	9.6	27.0	38.3	30.0	38.6	51.4	25.5	48.6	40.2
Male Loser 3	9.9	10.7	12.7	23.2	33.6	25.4	48.7	36.7	31.8	44.0
Female Loser 4	25.9	47.3	46.2	47.9	49.8	48.3	48.2	49.1	58.5	49.0
Female Loser 6	7.5	7.9	12.4	14.6	16.6	39.0	27.2	17.2	34.0	31.7
Female Loser 3	11.8	18.9	36.6	34.5	33.8	20.1	47.6	33.6	48.9	37.4
Mean	12.5	17.7	27.8	31.6	33.1	33.9	44.8	35.1	45.2	42.1
Animal	**31**	**32**	**33**	**34**	**35**	**36**	**37**	**38**	**39**	**40**
Male Winner 2	6.6	9.1	7.3	8.9	8.4	7.6	7.5	17.4	9.4	8.3
Male Winner 5	7.0	7.9	7.6	9.1	7.8	7.1	7.0	8.0	8.1	7.7
Male Winner 6	7.2	11.1	8.1	8.1	7.4	8.5	8.1	8.0	7.0	7.4
Female Winner 5	8.5	8.1	8.8	10.8	11.3	14.3	10.9	14.3	13.4	12.4
Female Winner 2	7.0	7.6	7.0	9.1	16.1	7.8	8.2	6.7	6.3	6.7
Female Winner 1	5.8	6.8	6.4	6.6	7.0	8.0	7.6	6.4	6.1	7.5
Mean	7.0	8.4	7.5	8.8	9.7	8.9	8.2	10.1	8.4	8.3
Male Loser 4	47.1	45.6	50.8	47.5	50.5	29.8	48.6	25.6	24.5	34.7
Male Loser 1	39.7	47.5	32.3	42.1	35.2	11.4	28.2	17.3	15.9	45.8
Male Loser 3	45.4	42.8	44.7	24.9	35.3	12.2	18.1	14.5	10.3	25.2
Female Loser 4	49.5	38.4	48.6	50.7	51.1	29.4	26.6	39.9	33.7	38.6
Female Loser 6	46.3	44.3	43.7	42.3	19.8	35.3	35.2	47.8	48.1	60.0
Female Loser 3	36.0	39.3	35.3	35.9	40.5	16.6	35.0	31.3	24.1	41.2
Mean	44.0	43.0	42.6	40.6	38.7	22.5	32.0	29.4	26.1	40.9

TABLE III—*Continued*

Animal	41	42	43	44	45	46	47	48	49	50
Male Winner 2	14.8	11.5	7.4	8.1	9.4	9.6	10.0	15.1	6.8	6.7
Male Winner 5	11.7	7.9	6.5	6.9	6.8	7.0	7.3	7.6	7.3	6.4
Male Winner 6	7.8	6.8	7.2	7.6	7.6	7.7	9.5	6.5	13.3	7.6
Female Winner 5	8.4	9.7	8.7	10.9	9.0	8.4	8.3	8.5	10.3	10.0
Female Winner 2	6.8	7.0	7.0	6.0	7.5	8.7	6.6	7.3	6.3	9.8
Female Winner 1	6.5	6.3	6.9	5.9	7.8	6.4	7.7	7.1	6.7	5.7
Mean	9.3	8.2	7.3	7.6	8.0	8.0	8.2	8.7	8.5	7.7
Male Loser 4	16.2	24.1	22.4	17.3	22.3	12.6	32.6	21.7	13.8	19.0
Male Loser 1	29.1	34.0	27.1	27.2	33.0	14.5	40.3	29.4	35.6	37.0
Male Loser 3	11.6	26.1	15.1	20.7	14.9	20.0	25.1	25.8	22.0	15.6
Female Loser 4	48.9	51.1	56.8	54.3	52.7	54.3	53.6	46.5	57.1	60.0
Female Loser 6	45.3	48.2	59.5	48.0	49.0	46.0	52.1	44.3	58.1	60.0
Female Loser 3	25.2	13.7	14.5	12.0	27.2	15.2	23.4	34.9	16.4	22.7
Mean	29.4	32.9	32.6	29.9	33.2	27.1	37.9	33.8	33.8	35.7
Animal	**51**	**52**	**53**	**54**	**55**	**56**	**57**	**58**	**59**	**60**
Male Winner 2	6.8	7.0	10.6	7.5	7.5	8.1	8.7	8.3	9.4	10.3
Male Winner 5	6.3	6.1	6.1	6.0	5.7	6.9	7.1	26.1	9.2	10.0
Male Winner 6	6.9	6.5	6.4	5.3	6.3	6.6	9.9	8.8	12.2	9.0
Female Winner 5	7.7	10.3	7.8	6.9	21.1	9.2	15.3	9.8	11.6	10.5
Female Winner 2	7.8	6.3	6.3	6.2	6.7	6.5	8.0	6.3	6.0	6.9
Female Winner 1	4.7	7.0	5.7	20.1	5.4	6.8	9.9	26.7	18.4	7.7
Mean	6.7	7.2	7.2	8.7	8.8	7.4	9.8	14.3	11.1	9.1
Male Loser 4	13.9	12.0	13.1	9.9	10.7	17.6	14.4	13.1	11.9	11.4
Male Loser 1	15.3	16.6	14.2	20.9	23.7	31.3	24.8	26.9	11.7	24.6
Male Loser 3	9.2	9.9	7.7	14.5	26.3	47.2	60.0	33.3	25.7	20.0
Female Loser 4	42.9	38.9	29.6	60.0	49.2	52.5	52.1	58.1	32.7	23.3
Female Loser 6	48.1	22.0	23.1	18.9	30.6	39.7	37.1	33.5	10.9	10.2
Female Loser 3	12.2	19.9	9.9	9.6	5.6	5.7	5.5	6.1	7.0	6.0
Mean	23.6	19.9	16.3	22.3	24.4	32.3	32.3	28.5	16.7	15.9

must be made with caution because it is possible that this small increase in speed of locomotion may represent nothing more than a later phase of a learning process. This caution is offered in spite of the apparent plateau that precedes.[2]

3. The results from the forty days of racing were unexpected and baffling. It was not anticipated that winners and losers would be segregated so quickly and consistently. As

[2] An experiment now in progress shows promise of contradicting the suggested conclusion that the rat's locomotor behavior is susceptible to social facilitation.

before stated, from the twenty-third day to the fifty-eighth day inclusive, the personnel of each group remained unchanged. The lower line which represents the performance of the winners in this phase of the graph reveals nothing notable except a further increase in speed and an apparently increased variability of performance at those points where the losers increased their speed. The upper line representing the performance of the losers rises rapidly to a maximum at about the thirtieth day and supposedly represents the effect of frustration resulting from the removal of the reward. From this point on, the graph falls gradually and very irregularly, indicating a slow increase in the running speed of the losers. It may be worthwhile to mention that during this period the percentage of refusals decreased from 50 percent to about 15 percent and on some days to zero. Casual observation of the losing animals during this phase of the experiment seemed to reveal first an increase and then a decrease in agitated behavior, as described by Hull (4) and Miller (5). These changes appeared to parallel the changes in running speed. As before stated, two female losers began winning during the last two days of the experiment. The author is at a loss to account for this latter phase of the loser's performance. It is indicated that some significant change has taken place among the factors determining the speed of locomotion. This change will not be identified without further experimentation but the author wishes to advance one highly speculative interpretation. It is not wholly unreasonable that the internal inhibitions generated by frustration have raised or released to a dominant status some unidentified motive, such as that of social facilitation or of escape.

Summary

It may be said in conclusion:

1. That if competitive behavior has been demonstrated it has not been identified.

2. That there is some evidence that the speed of locomotion in the white rat is susceptible to influence through social facilitation.

Bibliography

1. Bayer, E., Beiträge zur zweikomponenten theorie des Hungers, *Zeit. f. Psychol.*, 1929, **112**, 1–54.
2. Harlow, H. F., Social facilitation of feeding in the albino rat, *J. Genet. Psychol.*, 1932, **41**, 211–221.
3. Harlow, H. F. and Yudin, H. C., Social behavior of primates. I. Social facilitation of feeding in the monkey and its relation to attitudes of ascendance and submission, *J. Comp. Psychol.*, 1933, **16**, 171–185.
4. Hull, C. L., The rat's speed-of-locomotion gradient in the approach to food, *J. Comp. Psychol.*, 1934, **17**, 393–422.
5. Miller, N. E. and Stevenson, S. S., Agitated behavior of rats during experimental extinction and a curve of spontaneous recovery, *J. Comp. Psychol.*, 1936, **21**, 205–331.

THE VERBAL CONDITIONING OF THE GALVANIC SKIN REFLEX[1]

BY STUART W. COOK AND ROBERT E. HARRIS

University of Minnesota

A survey of the literature shows that the galvanic skin response has been conditioned in adults by the following investigators: Golla (1), Freeman (2), Switzer (3), Bass and Hull (4), Ischlondsky (5), Sorokhtina-Minut (6), Myasischev (7), and Slight (8). Apparently each of these investigators has considered the conditioning of this reflex, like that of other reflexes, to be the result of an experimental series of paired adequate and inadequate stimuli.[2] The possibility occurred to us that even without an experimental series, a conditioned galvanic response to an inadequate stimulus might be established by means of an association made by the subject between two verbalizations, for example, the verbalization 'light,' and the verbalization, 'receive a shock,' the latter being a previously conditioned stimulus for the galvanic response. It is such an establishing of the conditioned response without benefit of the experimental series that we have termed verbal conditioning.

If our hypothesis is correct, the subject having been instructed that an inadequate stimulus, the shining of a dim light, will be followed by an adequate stimulus, shock, the following predictions can be made:

1. The response to light will be present before a conditioning series (light-shock pairs) as well as afterwards.

[1] We are indebted to Professor Miles A. Tinker under whose direction this study was done, and to Dr. Starke R. Hathaway, Dr. B. F. Skinner, and Dr. Evelyn Raskin from whom we received helpful suggestions and criticisms.

[2] Freeman, who obtained conditioned responses after one to two trials, qualified his conclusion as follows, "Since the quantitative results were obtained in a manner similar to that of animal experimentation, I have interpreted them as 'conditioned responses,' but it seems that they might just as easily be interpreted as physiological accompaniments of the attitudes of expectation and surprise."

2. The number of responses occurring before experimental extinction will be independent of the number of conditioning trials.
3. When the subject is instructed that the light will no longer be followed by shock, there will be a more rapid extinction than is found in the absence of such instructions.

This study was designed to verify or disprove the above hypothesis by an experimental test of each of the predictions derived from it.

For measurement of the galvanic skin response, we used a Leeds and Northrup D'Arsonvol reflecting wall galvanometer, No. 2239-B, with a resistance of 1100 ohms. A beam of light was reflected from the mirror mounted on the swinging coil of the galvanometer to a translucent screen along which was placed a centimeter scale for measuring the magnitude of the response. The response was picked up by electrodes of polished silver held in place on the subject's right palm by a block of wood wound with a felt strap.

A current was maintained through the subject and through the galvanometer to which the subject was connected in series. Balancing this current was another through the galvanometer alone. In this latter circuit there was a fixed resistance of 100,000 ohms. The current through the subject and galvanometer, and hence the position of the reflected beam of light on the scale, could be regulated at will by manipulation of a variable resistance. A second variable resistance shunted the galvanometer and varied its sensitivity. The current through the subject, when the galvanometer reading was zero, was measured and found to be 14 microamperes.[3]

The current for the shock was obtained from an inductorium and conducted to copper electrodes placed on the left palm of the subject. A green light of subdued intensity was used as the inadequate stimulus. A red light of similar intensity was shone every ten trials to indicate rest periods, which were necessary to keep the subjects from becoming tired and restless during the experimental series.

The experimentation was conducted in a pair of adjoining rooms made completely soundproof to incidental noises by construction and by the humming of a pair of electric fans. All apparatus except the leads to the subject and to the lights was placed in one room, the subject being alone in the other.

The procedure described below was, in general, similar throughout the experiment. Within each of the three sections designed to test a particular prediction, it was identical for all subjects. Modifications in procedure for the different sections will be outlined in the discussion of the results.

The subject was seated facing the lights, a pair of electrodes on each palm. Since the normal impedance of one individual varies from that of another and it was desirable to have the magnitude of the response approximately equal for all subjects, it was necessary to regulate the sensitivity of the apparatus for each person tested. In order to do this, the size of each subject's response to such stimuli as a loud clap of the hands, or a slap on the chest was determined, and the resistance in the shunt altered until these stimuli evoked responses approximating a given magnitude.

[3] This circuit, which is a modification of the Wheatstone bridge, was suggested by Dr. Hathaway.

All subjects were given a preliminary series in which any response to the green light presented alone was extinguished. This extinction series was run before any mention of the shock was made or any shock given. In preliminary work we had discovered that if the subject knew that there was a possibility of his receiving painful stimulation, it was more difficult to extinguish any original response to the green light which might be present.

After this initial series of responses to the green light alone, a weak electric shock was given to the subject and raised by steps to a point where it was declared painful. It is interesting to note that this point in nearly every case corresponded to the point where adequately large and approximately equal responses were recorded. If, during an extended conditioning series, the magnitudes of the responses decreased, they were brought back to their former size by increasing the shock.

After the shock had been adjusted, the subject was given a conditioning series in which the inadequate stimulus, green light, was presented for three seconds, and followed immediately by the shock. To test for the presence of the conditioned response, the green light was shone alone. The strength of the conditioned response set up was determined by the magnitude and the number of responses observed before experimental extinction was complete. The magnitude of the conditioned response was arbitrarily taken as the average of the two largest responses to the light, with the exception that the first response and any response immediately following a rest period were ignored. These latter responses were not considered representative of the strength of the conditioning, since any change in procedure was found to be adequate to produce a sizeable reaction. Thus, several responses are listed in the tables to follow as having zero magnitude. This means that in the extinction series for these subjects, only one response, the first, occurred. On some trials, where the deflection occurred too quickly or where the latent period was too long, the response was recorded by a question mark. Half of these question marks were counted when determining the number of trials taken to extinguish the response. In every extinction series, four consecutive responses of zero magnitude were taken as the criterion of experimental extinction.

Thirty-five University of Minnesota men, most of them from elementary classes in psychology, were used as subjects. No special criteria of selection were employed. Seven subjects were discarded because their responses to the green light, the inadequate stimulus, could not be extinguished in an extended series of trials. Three additional subjects were discarded because they showed no evidence of conditioning as a result of either the instructions or an extended series of light-shock combinations. While the data obtained from these discarded subjects are significant, the fact that they offer neither positive nor negative evidence for the problem under consideration resulted in their exclusion from this report. All subjects stated when tested that they had no knowledge of the procedure. Most of the subjects knew what a conditioned reflex was and how it was established, but few knew anything about the galvanic skin reflex.

The results from the experimental test of each of the three predictions made from our hypothesis are presented and discussed separately. Averages are computed as a convenient means of indicating the trend of the results; but, in view of the small number of cases, most emphasis should be placed on the individual records. Such records are presented throughout the report.

Prediction 1.—The response to light will be present before a conditioning series (light-shock pairs) as well as afterwards.

To test this prediction the following procedure was devised. The six subjects in this part of the experiment have been designated Group I. After the routine measurement of the reaction to the inadequate stimulus, the green light, the subject was instructed that the green light was to be followed by a shock. To determine whether any conditioning had taken place during the reading of the instructions, the response of the subject to the green light alone (not followed by the electric shock) was tested for immediately.

Next, to determine what response to the inadequate stimulus would be present after a prolonged conditioning series, the same subjects were given, after a re-reading of the instructions, thirty presentations of the green light followed by shock. After this, the number and magnitude of responses to the green light alone before extinction were again determined.

Table I gives for each subject the results obtained from

TABLE I

COMPARISON OF RESPONSES TO GREEN LIGHT BEFORE INSTRUCTIONS, AFTER INSTRUCTIONS, AND AFTER 30 CONDITIONING TRIALS (GROUP I)

Subject	Number of Responses			Magnitude of Responses (Average of Two Largest)		
	Before Instructions	After Instructions	After 30 Trials	Before Instructions	After Instructions	After 30 Trials
Gu	1	2	2	3	63	17
He	7	5	21	12	21	74
Go	6	20	0	6	21	0
Br	1	23	6	0*	56	32
Th	8	18	16	10	15	10
Co	3	8	4	20	39	21
Av.	4.3	12.5	8.1	8	35.8	25.6

* See discussion of procedure.

both series of measurements. An inspection of this table indicates that our first prediction is correct. That conditioning has taken place without a so-called conditioning series is clearly apparent from a comparison of the average number of

responses made to the green light before and after receiving instructions. More significant than the absolute difference in averages is a comparison of the individual records. In four (*Go*, *Br*, *Th*, *Co*) of the six cases, there is a very marked difference in number of responses before and after the instructions. In subjects *Gu* and *He*, for whom there is no increase in number of responses, conditioning during instructions is shown by the substantial increase in magnitude of response.

A further proof that the establishment of the conditioned response is not a result of the conditioning series is found by examining the number and magnitude of responses given before experimental extinction of the reaction to the green light took place. With the exception of subject *He*, there is no increase in either number or magnitude of responses after the thirty experimental associations of light and shock. In fact, there appears to be a slight decrease, which, since these measurements were made at the end of a long experimental period, may possibly be attributed to experimental adaptation or to other factors which we are unable to isolate. Such factors may account for the otherwise inexplicable absence of a response in subject *Go*.

This increase in the response to the green light after the reading of the instructions, and the absence of a further increase in the strength of the response after the 30-trial conditioning series, both point to the fact that conditioning of the galvanic response occurs before the experimental association of the inadequate and adequate stimuli.

Prediction 2.—The number of responses occurring before experimental extinction will be independent of the number of conditioning trials.

This prediction was tested by comparing the number of responses to the green light alone (before experimental extinction), after conditioning series of 1, 15, and 30 trials. The usual instructions that the green light was to be followed by shock were given before the conditioning series. If conditioning occurred during the instructions, then the strength of the conditioned response should be as strong after one combination of light and shock as after thirty such combinations.

Three groups of subjects were used; Group II received one light-shock combination, Group III fifteen combinations, and Group IV thirty combinations. The results for each group are presented in Table II.

TABLE II

COMPARISON OF RESPONSES TO GREEN LIGHT AFTER ONE, FIFTEEN AND THIRTY CONDITIONING TRIALS

Subject	Number of Responses		Magnitude of Responses (Average of Two Largest)	
	Before Conditioning	After Conditioning	Before Conditioning	After Conditioning
Group II (1 trial)				
Zi	11	33	8	24
McM	17	31	9	11
Jo	1	26	0	28
Ma	3	18	8	25
Dy	2	24	3	11
Aver.	6.2	26.4		
Group III (15 trials)				
Fe	1	5	0	9
So	3	26	3	39
Ga	14	43	16	33
We	1	45*	0	23
Aver.	4.8	30		
Group IV (30 trials)				
Vo	14	28	10	15
McC	6	17	7	16
El	9	14	22	50
McMa	4	15	13	55
Br	2	12	2	20
Aver.	5.8	18		

* Stopped testing at this point since subject showed no sign of extinction.

The fact that, after 1, 15, and 30 trials, the average number of responses given to the green light before experimental extinction was 26.4, 30, and 18 respectively, shows that the strength of conditioning is independent of the number of light shock combinations given. However, there does

appear to be a difference between the average number of responses for these groups and for Group I, which was conditioned without an experimental series. Table I shows the average number of responses for the latter group to be 12.5. If this difference is significant (and there are several overlapping cases), it would seem that though the conditioned response is set up as a result of verbal conditioning, it is strengthened by a single presentation of the light-shock combination.

We conclude from the evidence in this part of the experiment that conditioning was completed during the instruction period or by the end of the first light-shock combination. Had the number of conditioned responses increased with the number of conditioning trials, then the conditioning would have been considered a result of a series of inadequate-adequate stimuli combinations. Failure to find such an increase indicates that the conditioning of the galvanic reflex differs in this respect from the customary conditioning of other reflexes.

Prediction 3.—When the subject is instructed that the light will no longer be followed by shock, there will be a more rapid extinction than is found in the absence of such instructions.

Our last prediction was checked by comparison of two groups of five subjects each. Group V (Table III) received the usual instructions and 30 light-shock combinations, after which they were told that the green light would then be shone without being accompanied by shock. Group IV, whose results have been recorded in Table II, was used as a control to furnish data on the rapidity of experimental extinction after a similar series of light-shock combinations without a change in instructions.

If a conditioned response has been set up verbally as a result of the acceptance of instructions, we would expect it to be greatly weakened or entirely eliminated (verbal inhibition) by additional instructions that the shock would no longer follow the light.

Table III shows clearly that this is what happens. The

TABLE III

RESPONSES TO GREEN LIGHT AFTER 30 CONDITIONING TRIALS PLUS INSTRUCTIONS THAT NO SHOCK WOULD FOLLOW (GROUP V)

Subject	Number of Responses		Magnitude of Responses	
	Before Conditioning	After Conditioning	Before Conditioning	After Conditioning
Wi.....	2	2	11	9
Fe.....	2	1	1	0
Hol.....	9	1	13	0
Hoe.....	1	0	0	0
Ne.....	23	3	16	8
Aver.....	7.4	1.4		

average number of responses after the change of instructions was 1.4, as compared to the average of 18 responses for the group who received no additional instructions. Again, more significance should be attributed to the fact that the individual records show that, after conditioning, no subject in Group V gave more than three responses to the green light, while no subject in the control group, Group IV, gave less than twelve responses.

CONCLUSIONS

This study has attempted a verification of the following hypothesis: the conditioning of the galvanic skin response in the human adult differs from the customary conditioning procedure in that, under the conditions of this experiment, (*a*) this response is established by means of a process of verbal conditioning, and (*b*) it is therefore not established as a result of a series of paired inadequate-adequate stimuli combinations.

The experimental findings confirming this hypothesis are as follows:

1. A conditioned galvanic response was obtained by merely instructing the subject that electric shock (adequate stimulus) would follow a green light (inadequate stimulus).

2. The actual presentation of a series of inadequate-adequate stimuli combinations did not increase the strength of the conditioned response beyond its strength following the verbal association made during the instructions.

3. No consistent difference in strength of conditioned response was found after conditioning series of 1, 15, and 30 trials.

4. Despite a conditioning series of 30 light-shock combinations, either a marked weakening or complete elimination of the conditioned response resulted from instructing the subject that the shock would no longer follow the light.

We conclude, therefore, that the galvanic skin reflex can be conditioned verbally. During preliminary work in which we attempted to condition this response by the usual laboratory procedure, it was found impossible to prevent such verbal conditioning from taking place. In view of the fact that former experimenters have failed to report taking effective precautions against the occurrence of similar phenomena in their own work, the interpretation of their findings should be modified in the light of the results reported here.

(Manuscript received February 4, 1937)

References

1. Golla, F. L., Objective study of neurosis, *Lancet*, 1921, **2**, 215–221.
2. Freeman, G. L., The galvanic phenomenon and conditioned responses, *J. Gen. Psychol.*, 1930, **3**, 529–539.
3. Switzer, St. C. A., Disinhibition of the conditioned galvanic skin response, *J. Gen. Psychol.*, 1933, **9**, 77–100.
4. Bass, M. J. and Hull, C. L., The irradiation of a tactile conditioned reflex in man, *J. Comp. Psychol.*, 1934, **17**, 47–65.
5. Ischlondsky, N. E., Contribution a la méthode d'étude objective de l'activité neuropsychique. Une nouvelle reaction pour l'investigation de l'activité reflexe conditionelle, *Bull. Acad. Med.*, 1930, **103**, 305–308.
6.* Sorokhtina-Minut, O., Inhibition and 'Freeing' of basal electrodermal reflexes, *Psychol. Abstr.*, 1930, **4**, 484.
7.* Myasischev, V., The psychogalvanic reflex and its significance in the study of personality, *Psychol. Abstr.*, 1930, **4**, 484.
8.* Slight, D., The conditioned psycho-galvanic reflex. Sir Frederick Mott Memorial Volume, 1929, Vol. 1 (as reported by Landis, C., Electrical phenomena of the skin, *Psych. Bull.*, **29**, 693–752).

* These references, being unavailable in the original, are cited from abstracts or secondary sources.

THE EFFECT OF BINAURAL OCCLUSION OF THE EXTERNAL AUDITORY MEATI ON THE SENSITIVITY OF THE NORMAL EAR FOR BONE CONDUCTED SOUND

BY NOBLE H. KELLEY, Ph.D., AND SCOTT N. REGER, Ph.D.

The State University of Iowa

The phenomenon of hearing sound conducted through the cranial bones was known as early as the sixteenth century. Phillipus Ingrassia (1510–1580) was one of the first to report the discovery (**11**, p. 88; ***5**).[1] Hieronymus Capivacci, writing in 1589, made the first practical application of bone conduction to clinical otology. He placed one end of a rod between the teeth of a patient with impaired hearing and the other end in contact with the vibrating string of a musical instrument, the zither. If the patient heard the tone, the deafness was diagnosed as a disease of the drum membrane; if he did not hear the tone, as a disease of the labyrinth (**11**, p. 143; ***1**). It was Schellhammer, however, who in 1684 experimentally proved that sound conveyed through the teeth was transmitted by the cranial bones and not by way of the Eustachean tube. This was accomplished by holding the stem of a vibrating fork in the closed mouth between the teeth, in one instance touching, in the other, not touching the teeth (**11**, pp. 213–214; ***17**).

The nineteenth century marked the beginning of more significant revelations concerning the problem of bone conduction. In 1827 Wheatstone observed that under certain conditions one's sensitivity for bone conducted sound is increased (**20**, pp. 67–69). He noted that, if the entrance to the external auditory meatus be closed without pressure by the finger, the sounds of the voice are greatly augmented. Placing the conducting stem of a sounding tuning fork on any part of the head, with the ears closed as above indicated, he

[1] See bibliography for explanation of the asterisk.

noticed a similar augmentation of sound. If one ear remained open, the sound was referred to the closed ear. Similarly, if the meatus and concha of one ear be filled with water, the sound of the fork was referred to the filled ear. These observations were confirmed by E. H. Weber in 1834 (**14**; ***19**). In 1855 Rinné observed that the normal ear heard a tuning fork longer by air conduction than by bone conduction. But if a lesion existed in the sound conduction apparatus, the sound was heard longer by bone than by air conduction (**14**; ***15**). Schwabach, however, was the first to state that, in case of obstructions in the sound conduction mechanism, a vibrating fork held in contact with the cranial bones is heard longer than in the normal ear. Whereas, when a lesion exists in the inner ear, the perception time is shortened (**13**, p. 153; ***18**). This marks the beginning of the theory that there is increased bone conduction in ears with conductive lesions.

Several experimental investigations have been made with tuning forks on the problem of 'increased' bone conduction in the normal ear. Runge (**16**) in 1923 repeated the water-filling experiments of Wheatstone, and found that if the normal ear be filled with water, the duration of perception of the sound of a tuning fork by bone conduction is doubled for frequencies up to 512 c.p.s. Above this frequency the reaction was not so pronounced. Fowler (**3**) in 1925 studied the effect of closure of the external meati on bone conduction acuity. Using a 256 c.p.s. tuning fork, the damping constant of which was known, he found that occluding the ears lowered the threshold for bone conducted sound by about 14 to 16 sensation units (decibels). Hallpike, in 1930, reports a study in which he concludes that there is little after-perception on occlusion of the meati. Since his study was made in the silence room of the Royal Ear Hospital, London, he advanced the explanation that the increased bone conduction reported in other studies was due to the 'screening' effect that occlusion had on external noise stimuli (**4**). Dean (**2**) also expressed a similar interpretation.

Pohlman and Kranz, recognizing the inadequancy of

tuning forks, approached the problem by means of a vacuum tube oscillator and a special bone conduction receiver. The data secured from a series of three investigations (**8**, **9**, **10**) showed marked variations. But they found, in general, that occluding the external meati with wax lowered the threshold for bone conducted sound for frequencies ranging from 128 to 1024 c.p.s. At 2048 c.p.s. occlusion had little or no effect.

Knudsen and Jones (**7**, p. 490) found that in a sound-proof room, when the canal is occluded with wax, the acuity for bone conduction is increased 10 db.

The Problem and Technique

This study represents a further attempt to investigate under carefully controlled conditions the phenomenon of 'increased' bone conduction in the normal ear. Certain limitations of previous investigations may be cited: first, the small number of observers used; second, the conditions under which the observations were made; and third, the deficiencies of the instruments available. Tuning forks, as well as the bone conduction receivers used in the past, are unsuitable for making measurements at the high frequency levels because the intensity at these levels is greater for air than for bone conducted sound. Under such conditions the experimenter cannot be certain whether the sound stimulus is heard through the cranial bones or through the medium of air.

The observations in this study were made in a sound-proof room, thus excluding the effect of extraneous noise. If this precaution is not taken, the auditory threshold with the ears open is raised by the extent of the noise level of the room. With the ears occluded the effect of extraneous room noise is excluded. Thus an artifact is introduced unless all measurements are made in a quiet environment.

The source of sound in this study was a Western Electric 2-A audiometer, which produces eight frequencies, in octave intervals ranging from 64 to 8192 c.p.s. The attenuation network of the instrument is calibrated in steps of five db. This permitted a fairly adequate control of both the frequency and the intensity of the tones generated. The bone conduction receiver used in the experimentation was a vibrating button type oscillator, part of a Sonotone bone conduction hearing aid.[2] A flexible metal

[2] Acknowledgment is gratefully made to the Sonotone Corporation, 19 W. 44th Street, New York City, for the loan of the bone conduction receiver used in this study.

head band attached to the receiver kept this instrument at a definite position on the skull and insured a relatively constant pressure throughout the testing procedure.

Twenty-five observers (50 ears), whose hearing was within the normal range, were used in the investigation. The external meati of each observer were examined, using a speculum, to make certain that the canals were not already occluded with cerumen. The auditory acuity of each observer for bone conducted sound was determined under two conditions: first, with the ears open; and second, with the ears occluded. Occlusion was made with cotton and vaseline. A small pellet of cotton was placed at the entrance to the external meati to prevent the vaseline from entering the canal, and the concha then filled level with vaseline. Measurements were made from two locations. First, the bone conduction receiver was placed over the mastoid portion of the temporal bone, in a plane with the upper border of the external acoustic meatus, and about 2.5 cm posterior to that. Measurements were taken from both the left and the right mastoid bones. The measurements from the two mastoids are combined in the presentation of the data. The second location of the receiver was on the mid-line of the forehead, just superior to the glabella.

Results

A. Effect of Bilateral Occlusion

Bilateral occlusion increased the auditory sensitivity for bone conducted sound as expressed in Tables I and II. The mean extent of the lowering of the threshold is expressed in decibels for each frequency measured. The average deviation of the individual measures was less than 5 db, the magnitude of the interval of calibration on the audiometer.

TABLE I

Bone Conduction Receiver on Mastoids

Frequency c.p.s.	Threshold Lowered—db
64	25
128	26
256	24
512	20
1024	17
2048	3
4096	1
8192	1

TABLE II

BONE CONDUCTION RECEIVER ON FOREHEAD

Frequency c.p.s.	Threshold Lowered—db
64	*
128	27
256	23
512	18
1024	14
2048	1
4096	0
8192	0

* The maximum intensity level obtainable from the audiometer for the 64 c.p.s. tone was at the reading of 50 db on the dial. With the ears open and the receiver on the forehead, no observer heard the tone at this output. With the ears occluded the mean level at which this tone was heard was at a reading of 31 db. This indicates that the threshold was lowered by an amount not less than 24 db.

Occlusion of the meati raised the threshold for air conducted sound by values ranging from 30 to 40 db. The threshold at the three highest frequencies was raised more than at the lower frequencies.

B. Forehead-Mastoid Decrement

Forehead-mastoid decrement refers to the decrease in intensity of sound due to propagation through the cranial bones from the mastoid to the forehead. This is determined by ascertaining the difference between the threshold values with the bone conduction receiver on the forehead and then on the mastoid. A consideration of trans-cranial decrement values is very important in making auditory threshold measurements for bone conducted sound. For instance, let it be assumed that the right cochlea is normal and that the left cochlea is normal with the exception of a loss of 50 db for the 4096 c.p.s. tone. With the bone conduction receiver on the left mastoid, this loss would not be indicated unless the right ear were masked with an interfering tone. Without this precaution, the sound from the left mastoid would be propagated to the right cochlea and there heard at an intensity level differing from the measurement at the right mastoid only by the amount of trans-cranial decrement. From the measurements made in this study it was possible to compute

the forehead-mastoid decrement both with the ears open and occluded. An examination of Table III reveals that the decrement was practically the same with the ears open as it was when they were occluded. This substantiates the fact that with the ears open only bone conducted sound was heard from the receiver. Additional precautions were taken to insure this (6).

TABLE III

FOREHEAD-MASTOID DECREMENT

Frequency c.p.s.	Ears Open db	Ears Occluded db
64	*	12
128	14	13
256	13	14
512	11	13
1024	7	10
2048	5	7
4096	6	7
8192	10	11

* The 64 c.p.s. tone could not be heard from the forehead at the maximum intensity obtainable from the auidometer with the ears open.

CONCLUSIONS

1. The phenomenon of 'increased bone conduction' is present in the normal ear when occluded.

2. The effect of binaural occlusion of the meati is a function of the frequency of the stimulus tone.

Occlusion lowered the threshold (increased the sensitivity) over the frequency range from 64 to 1024 c.p.s. From 2048 to 8192 c.p.s. occlusion had no significant effect.

The effect of occlusion was approximately the same with the bone conduction receiver on the forehead as when on the mastoids.

3. The forehead-mastoid decrement, under the conditions of the study, also varied with frequency of the stimulus tone.

The decrement ranged from 10 to 14 db for the eight frequencies, with the exception of 1024, 2048, and 4096; at which frequency levels the decrement was approximately 7 db.

(Manuscript received January 18, 1937)

References[3]

*1. Capivacci, Hieronymus, *Opera omnia quinque sectionibus comprehensa*, cap. 1, pp. 587–591, De laeso auditu, 1589.

2. Dean, C. E., Audition by bone conduction, *J. Acoust. Soc. Amer.*, 1930, **2**, 281–296.

3. Fowler, E. P., Fundamentals of bone conduction, *Arch. Otolaryng.*, 1925, **2**, 529–542.

4. Hallpike, C. S., Some observations on bone conduction, *J. Laryng. & Otol.*, 1930, **45**, 1–24.

*5. Ingrassia, Phillipus, *In Galeni librum de ossibus doctissima et exspectatissima commentaria, Panormi, ed. post mortem*, 1603.

6. Kelley, N. H., The effect of binaural occlusion of the external auditory meati on the hearing acuity of the normal ear for bone conducted sound, *State Univ. of Iowa thesis*, 1935.

7. Knudsen, V. O. and Jones, I. H., Bone conduction, *Arch. Otolaryng.*, 1931, **13**, 489–505.

8. Pohlman, A. G. and Kranz, F. W., The problem of middle ear mechanics; Chapter III, Binaural acuity for air and bone transmitted sound under varying conditions in the external auditory canal, *Ann. Otol., Rhin. & Laryng.*, 1925, **34**, 1224–1248.

9. Pohlman, A. G. and Kranz, F. W., The influence of partial and complete occlusion of the external auditory canals on air and bone transmitted sound, *Ann. Otol., Rhin. & Laryng.*, 1926, **35**, 113–121.

10. Pohlman, A. G. and Kranz, F. W., Monaural tests on bone acuity under various conditions, with some general comments on bone acuity, *Ann. Otol., Rhin. & Laryng.*, 1926, **35**, 632–641.

11. Politzer, A., *Geschichte der Ohrenheilkunde*, Stuttgart, Enke, volume 1, 1907.

12. Politzer, A., *Geschichte der Ohrenheilkunde*, 1850–1911, Stuttgart, Enke, volume 2, 1913.

13. Politzer's *Textbook of the Diseases of the Ear*, sixth edition, revised by Milton J. Ballin, Philadelphia, Lea and Febiger, 1926.

14. Report of Committee for the Consideration of Hearing Tests, *J. Laryng. & Otol.*, 1933, **48**, 22–48.

*15. Rinné, F. H., *Präger Vierteljahreschrift für Heilkunde*, 1855, **1**, 72; and **2**, 45–155.

16. Runge, H. B., Über die Lehre von der Knockenleitung und über einen neuen Versuch zu ihrem weiteren Ausbau, *Zeitschrift f. Hals-, Nasen- u. Ohrenheilk.*, 1923, **5**, 289–403.

*17. Schellhammer, G., *De auditu liber unus.* Quo pleorumque omnium doctorum sententiae examinatur, et auditus ratio nova methodo, ex ipsius naturae legibus, explicatur, Lugdunum, Batavorum, 1684.

*18. Schwabach, D., *Zeitschrift für Ohrenheilkunde*, 1884–85, **14**, 61–148.

*19. Weber, E. H., *De pulsu resorptione, auditu and tactu*, Lipsiae, 1834.

20. Wheatstone, C., Experiments on audition, *Quart. J. of Science, Literature and Art*, July–Dec., 1827, 67–72. See also The Scientific Papers of Sir Charles Wheatstone, published by the Physical Society of London, Taylor and Francis, 1879, Chapter 4, Experiments on Audition.

[3] The asterisk preceding a reference indicates that it was secured from a publication investigated by the authors but was not itself directly examined.

QUANTITATIVE CORRELATES OF EUPHORIA *

BY W. A. BOUSFIELD AND H. BARRY, JR.
Tufts College

In an earlier paper, the writers (1) have justified the use of the term, euphoria, to designate the state of general well being or what has been conventionally described as feeling tone. The simple technique consisted of having subjects rate their present euphoria on a plus five to minus five basis. Euphoria was defined as "how well you feel; plus five being as well as possible and minus five being as badly as possible." We observed that in spite of individual variations from time to time, group averages were significantly related to the pooled data of several other variables. The most definite of these were sleep, and the presence or absence of worry. In the case of the former, the highest average euphoria was reported for 8 to 8¾ hours of sleep during the previous night. Amounts of sleep below or above this optimum were associated with definitely lower average euphoria. The subjects who admitted they were worried had, in every group investigated, a definitely lower euphoria.

The most obvious of the various alternatives for continuing the research was to find some objective correlate to these highly subjective judgments. A survey of the literature yielded relevant information. One of the perennial subjects of controversy since affectivity was taken over by the experimentally minded has been the memory value of pleasantness as compared with that of unpleasantness. For reviews of the long series of experiments we would refer the reader to Meltzer (5), Beebe-Center (2), Cason (3), and Moore (7). The majority of these researches have supported the view that pleasantness favors retention more than does unpleasantness. Certain of the investigators, however, have ventured a further

* Adapted from a paper entitled, Quantitative Correlates of Feeling Tone, read at the 44th annual meeting of the American Psychological Association, September, 1936.

step. Meltzer (**6**), and Cason (**3**) have classified their subjects as optimists or pessimists, according to the relative preponderance of pleasantly or unpleasantly toned items in their memory productions. There remains a need here of precisely defining the terms, optimist and pessimist. It would also be desirable to determine the correlation between the relative number of pleasantly and unpleasantly toned items and some criterion that would follow from the definitions of optimist and pessimist. Laird (**4**) attempted to secure the necessary validation on the basis of subjective ratings. The adequacy of these ratings, however, is problematical.

Admitting the need for further study in this connection, the results of these experiments strongly suggest that high euphoria should be associated with the tendency to give productions of a pleasant nature. A technique was devised for testing this proposition. Groups of subjects, provided with blank paper, were given the following instructions by the experimenter:

1. Please write your name on the paper.

2. The next item is the rating of your euphoria, or how well you are feeling at the present time. The scale of rating varies between plus five and minus five, plus five being as well as possible and minus five being as badly as possible. Estimate your rating on this scale as closely as you can.

3. In this part of the experiment you are simply requested to write as many pleasant and unpleasant items as you can. These can be objects, activities, situations, or anything that suggests pleasantness or unpleasantness to you. Possible pleasant items might be ice-cream, winning a game, vacation, friends, and money. Possible samples of unpleasantness might be headache, loud noises, being thought stupid, and falling. Make your lists as long as possible. When the signal is given, start making a list of pleasant items.

Intervals of 1½ minutes, alternately spaced, were allowed for listings in the two categories, the order being 1st, P *items*; 2nd, U *items*; 3rd, P *items*; and 4th, U *items*, In other words, the total time allowed for each category was 3 minutes.

Sample data will serve to illustrate the nature of the items listed. Subject N. N. F.: Euphoria + 5; *P items*, riding, walking, eating, resting, looking at trees, conversation with a friend, smelling of flowers, working, laughing, reading, joking, shopping, freshening up after shopping, looking over a magazine. *U items*, headache, nausea, argument, tiredness, loss of appetite, pains, financial worry. Another subject, M. R. F., with a euphoria of + 1, gave the following productions: *P items*, golf, winning match, watching baseball game, watching football game, playing ice hockey, skating. cold drink, good food, vacation, fishing, good show or movie, seasonable weather, pleasure driving. *U items*, late for train, late to class, examinations, writing thesis, flat tire, poor sportsmanship, bad show, engine trouble, bodily ailments or illness, poor food. A third example shows that the productions may be influenced by physical

ailment. Subject A. S. P.: Euphoria − 5; *P items*, seeing my girl friend, eating ice cream, relief from my present tooth trouble, candy, 10 gallons of free gasoline, a good drink of cool milk, my quarterly payment, money, a good movie, a job given to me, not to have to shave, today being tomorrow, a letter from Philadelphia. *U items*, my tooth trouble, the damned noise in my radio, work tonight, cleaning the kitchen, having no money, going to psychology class, having no gas in my car, having to read for English, my girl friend is away, no meat for dinner, having a tooth out, having to reline my brakes, having to polish a car, having to listen to lousy radio program, the fellow I work with, have to go to Hartford tomorrow, not graduating in June.

The treatment of the data involved the making of summations of all the pleasant items recorded by each subject, and then all the unpleasant items. Next, the ratios of pleasant items to unpleasant items were determined for each individual subject. We have designated these ratios as P/U. The average euphorias were calculated for 5 different ranges of P/U ratios. Undergraduate students in psychology supplied the data. Results from 400 subjects, 260 men and 140 women, are as follows:

P/U Ratios	Av. Euphoria	P. E. (av.)	Cases
1.90–up	+3.18	0.12	44
1.60–1.89	+2.37	0.16	67
1.30–1.59	+2.22	0.13	105
1.00–1.29	+1.95	0.12	138
0–0.99	+1.17	0.26	46

Av. Euphoria of Men = + 1.98 ± 0.12
Av. Euphoria of Women = + 2.44 ± 0.09
Av. P/U Ratio of Men = 1.41 ± 0.02
Av. P/U Ratio of Women = 1.46 ± 0.03

The pooled data give unmistakable evidence of a relationship between average euphoria and the ratio of pleasant to unpleasant items. The higher average euphoria reported by the women confirms a similar finding in our earlier study. We can see, however, that the difference between the average P/U ratios for the men and women is both small and unreliable. As yet we have insufficient information to account for the failure of the women to give a greater preponderance of pleasant items.

The positive findings of this study are significant from several points of view. First of all, there is evidence for the

validity of the self-ratings of euphoria. This validity follows from the direct relationship between euphoria and the relative preponderance of pleasant over unpleasant productions. The fact that we depend on averages to demonstrate the relationship does not invalidate the point, for the manifold variables inherent in this type of research are to be expected. The significance follows from the fact that positive data emerge in spite of the operation of factors that tend to dilute and obscure the trends that concern us. But we may proceed further with the conclusions. Euphoria is properly regarded as an attitude, for we have evidence of a general determining tendency capable of modifying specific aspects of behavior. In this experiment, we have shown that it controls the differential production of affectively toned items. The individual who reports high euphoria draws more heavily on his pleasantly toned experiences. Insofar as we can speak of a pleasant mental content, we are justified in assuming that a large proportion of its determiners rank high in affective value. We should point out here that we have been unable to find any significant relationship between the euphoria ratings and the total number of items (the sum of the pleasant and the unpleasant) enumerated in our data. It would therefore appear that the facilitation of the pleasant productions is accompanied by some sort of simultaneous inhibition of the unpleasant. Still another fact of interest is that the preponderance of positive euphorias cannot be due solely to a simple factor such as over-estimation, for the typical P/U ratio is considerably above unity. It is significant that only 11.5 percent of the P/U ratios were below one, and furthermore only 12.5 percent of all the euphorias reported were negative.

Conclusions

The technique of obtaining spontaneous productions of a pleasant and unpleasant nature yields data that give validity to ratings of euphoria or feeling tone. When averages are considered, euphoria varies directly with the ratio of the pleasant to the unpleasant items. Thus, the individual who reports high euphoria tends to make a relatively large number

of pleasant productions and a relatively small number of unpleasant ones. The items reported by an individual with a lower euphoria, on the other hand, include relatively fewer that are pleasant and relatively more that are unpleasant. Euphoria is properly regarded as an attitude. This attitude may be made the object of definite experimentation.

(Manuscript received February 2, 1937)

References

1. Barry, H. and Bousfield, W. A., A quantitative determination of euphoria and its relation to sleep, *J. Abn. and Soc. Psychol.*, 1935, **29**, 385–389.
2. Beebe-Center, J. G., *The psychology of pleasantness and unpleasantness*, N. Y., Van Nostrand, 1932, 354–384.
3. Cason, H., The learning and retention of pleasant and unpleasant activities, *Arch. of Psychol.*, 1932, **21**, No. 134, 1–96.
4. Laird, D. A., The influence of likes and dislikes on memory as related to personality, *J. Exper. Psychol.*, 1923, **6**, 294–303.
5. Meltzer, H., The present status of experimental studies of the relationship of feeling and memory, *Psychol. Rev.*, 1930, **37**, 124–139.
6. Meltzer, H., Individual differences in forgetting pleasant and unpleasant experiences, *J. Educ. Psychol.*, 1930, **21**, 399–409.
7. Moore, E. H., A note on the recall of the pleasant vs. the unpleasant, *Psychol. Rev.*, 1935, **42**, 214–215.

AN EXPERIMENT TO DETERMINE HOW ACCURATELY COLLEGE STUDENTS CAN INTERPRET THE INTENDED MEANINGS OF MUSICAL COMPOSITIONS

BY MELVIN RIGG

Kenyon College

Is the best of our operatic and program music really descriptive of the action which it is supposed to portray? Or could the music of a song of farewell have been set, just as appropriately, to the words of a love lyric or a prayer? Music enters readily into associative bonds. A piece which the writer practiced as a boy always suggests Egypt to him, since he was reading a book at the time dealing with this country—in spite of the fact that the composition is from a Polish composer with nothing Egyptian about it. It is true consequently that when we know the traditional meaning of a selection, the appropriateness of the musical setting seems natural to us. Mendelssohn's Wedding March will make us think of orange blossoms, and the Star Spangled Banner of patriotism. We have never heard the music of the latter in its original form as a drinking song.

In order to test how accurately the intended meaning of music can be judged in the case of unfamiliar selections, the writer played eighteen Victrola records to auditors, who recorded their impressions. In each case, the entire record was heard. The eighteen were consequently divided into two series of nine records each, and the two series presented at different times to avoid fatigue. In addition to the blank for the responses, the following outline was provided each auditor for his guidance.

I. Sorrowful, serious, religious, etc.
  A. Death
    1. Death scene
    2. Funeral March
    3. Elegy

B. Sorrow
4. Sorrow in general
5. Farewell
C. Religion
6. Good Friday Music
7. Prayer

II. Joyful (Love, Activity, Nature, etc.)
D. Love
8. Love Song
9. Serenade
E. Activity
10. Spinning Song
11. Cradle Song
12. Dance
F. Nature
13. Morning
14. Moonlight

This outline was made to accommodate the actual selections that were used; it is not designed as a universal system of classification for all music. The auditors were asked to record first whether the music was predominantly sad-serious or joyful, and they entered either I or II on their blanks. Then they were to try to make a more accurate judgment and tell whether the piece portrayed death, sorrow, religion, love, activity, or nature. They did this by entering on their reports one of the capital letters from A to F. After that a still finer discrimination was to be attempted, and the subjects were to indicate whether the record represented a death scene, a funeral march, an elegy, etc., by using the arabic numerals from 1 to 14. Finally, the auditors were asked if they had heard the composition before, and what the name of it was. Most of the music was not recognized, and in only one percent of the cases could the name be given correctly. When an auditor knew the name, his reply was thrown out, since the judgment would be influenced by his previous knowledge. Occasionally a word or two of the songs would be discriminated, but most of the vocal numbers were in foreign languages, and the one which was English was sung by a foreigner, so this source of error was not extensive. The subject was asked, when he heard a significant word, to record the fact, and his response to this item was not included in the tabulation.

The auditors in this experiment were first year psychology students in a men's college. They included all classes from freshman to senior, and persons of various degrees of musical training.

The following table gives the name of each selection, the total number of replies, the correct classification and the number of responses in each category.

Seventy-three percent of the auditors can tell whether the music is sad or joyful. Forty-one percent can further classify the pieces into the categories: death, sorrow, religion, love, activity, and nature. Only twenty-five percent can give the still finer classification as indicated on the outline. The percentages are in each case better than chance, but in

TABLE I

TOTAL NUMBER OF RESPONSES, THE CORRECT CLASSIFICATION OF THE RECORDS, AND THE NUMBER OF RESPONSES IN EACH CATEGORY

	Total Number	First Discrimination	Number Right	Second Discrimination	Number Right	Third Discrimination	Number Right
Series A							
Siegfried's Funeral March	71	I*	68	*A**	54	No. 2*	15
Butterfly's Death Scene	71	I	61	*A*	10	No. 1	6
Swedish Cradle Song	61	II	56	*E*	44	No. 11	39
Massenet's Elegy	70	I	17	*A*	4	No. 3	3
William Tell Overture, At Dawn	71	II	19	*F*	15	No. 13	11
Tschaikowsky, Adagio Lamentoso	71	I	51	*B*	17	No. 4	14
King's Prayer, Lohengrin	72	I	44	*C*	29	No. 7	20
Garden Music, Faust	70	II	58	*D*	58	No. 8	39
Turiddu's Farewell	69	I	46	*B*	26	No. 5	19
Series B							
Fatal Stone, Aida	71	I	61	*A*	8	No. 1	5
Moonlight, Debussy	69	II	63	*F*	25	No. 14	15
Farewell, La Boheme	72	I	26	*B*	19	No. 5	11
Elizabeth's Prayer, Tannhauser	73	I	47	*C*	25	No. 7	16
Good Friday Music, Parsifal	73	I	44	*C*	11	No. 6	5
Death of Boris Godounow	74	I	55	*A*	30	No. 1	13
Omphale's Spinning Wheel	72	II	72	*E*	53	No. 10	36
Serenade, Don Giovanni	73	II	72	*D*	42	No. 9	28
Death and Transfiguration	74	I	66	*A*	57	No. 1	27
Totals, both series	1277		926		527		322
Percentages right			73%		41%		25%
Chance distribution would be Average N	71		50%		17%		7%

* See the previously given outline for the meaning of these symbols.

only the first discrimination is there a majority of responses for the right category. The data seem to warrant the assertion that students can discriminate between sad and joyful music, since the significance ratio of this proportion of 73 percent is 4.36.[1]

[1] The following mathematical treatment was suggested by Dr. H. A. Edgerton.

$$\sigma_{\text{pro.}} = \sqrt{\frac{pq}{N}},$$

where p is the proportion in question and q is $1 - p$. Therefore,

$$\sigma_p = \sigma_q,$$

$$\sigma_{\text{diff.}} = \sqrt{\sigma_p^2 + \sigma_q^2 - 2\sigma_p\sigma_q r_{pq}}.$$

But $\sigma_q^2 = \sigma_p^2$ and $r_{pq} = -1$.

Therefore

$$\sigma_{\text{diff.}} = \sqrt{2\sigma_p^2 - 2\sigma_p^2(-1)},$$

$$\sigma_{\text{diff.}} = \sqrt{4\sigma_p^2} = 2\sigma_p.$$

The percentage of correct responses for each of the various categories taken separately is as follows:

TABLE II

CORRECT RESPONSES ARRANGED BY CATEGORIES

Category	No. of Items	Percentage of Correct Responses
First Discrimination		
I. Sad-serious	12	68%
II. Joyful	6	82%
Second Discrimination		
A. Death	6	38%
B. Sorrow	3	29%
C. Religion	3	30%
D. Love	2	70%
E. Activity	2	73%
F. Nature	2	29%
Third Discrimination		
1. Death Scene	4	18%
2. Funeral March	1	21%
3. Elegy	1	4%
4. Sorrow in general	1	20%
5. Farewell	2	21%
6. Good Friday music	1	7%
7. Prayer	2	25%
8. Love song	1	56%
9. Serenade	1	38%
10. Spinning song	1	50%
11. Cradle song	1	64%
12. Dance	Not represented	
13. Morning	1	15%
14. Moonlight	1	22%

The love and activity categories are the best discriminated. There are only two love items, the Garden Music from Faust, which may have been correctly placed because it is a duet for a male and a female voice, and the serenade, in which the characteristic accompaniment may have helped. There are

---

The significance ratio, S. R., is $\frac{p - q}{2\sigma_p}$.

For this particular proportion the values are

$$\frac{.73 - .27}{2\sqrt{\frac{.73 \times .27}{71}}}$$

S. R. = 4.36.

The proportion is therefore significant.

likewise two activity items, the spinning song and the cradle song, both of which were correctly classified by a large number of judges. In the spinning song the rhythm is suggestive. Of the Death items, Strauss' Death and Transfiguration was the most successfully placed (by 36 percent).

The incorrect classifications are also interesting. Butterfly's Death Scene was generally regarded as a song of farewell. It may be claimed that this is not a serious error, but the point to be noted is that in this instance at least persons do not differentiate a death scene from a simple farewell. Massenet's Elegy was classified as a love song or as a serenade. If grief was intended to be represented, the attempt was not in this case successful. At Dawn, a portion of the William Tell Overture, was by more than half of the judges regarded as expressing some sort of sorrow. Twenty judges spotted the Lohengrin prayer, but nineteen other judges thought it was a serenade. The Fatal Stone from Aida was generally considered as a farewell, again not a serious mistake, although Aida dies on the stage at the end of the scene. The Farewell from La Boheme was put down by almost half of the auditors as a love song; the sorrow of the selection was not recognized. In the case of the Good Friday music from Parsifal, twenty-three judges thought it was sorrowful (but not religious); on the other hand thirteen thought it to be a serenade. The real serenade was interpreted by twenty-three judges to be a dance. Death and Transfiguration was correctly called a death scene by twenty-seven auditors, and a funeral march by twenty-five.

The number of placements in correct categories seems low in view of the fact that the students were supplied with outlines, which gave them a clear idea of what was coming. It might be too much to expect anybody hearing music at random suddenly to realize that a selection was appropriate for Good Friday. But when 'Good Friday Music' is one of the fourteen headings on an outline, the fact that the number of correct responses is just about the chance distribution assumes a different aspect.

The writer's placement of the pieces may, however, be

open to question in some cases, and since he would not wish his conclusions to be based on an artifact of classification, it might be wise to see what would happen under a more flexible interpretation. In determining the correct responses to the second discrimination, the following relaxations in scoring seem to be the most logical.

Selections	Extra Responses Counted Correct	Number
Aida, Fatal Stone	Sorrow	47
Elegy	Sorrow	11
Adagio Lamentoso	Death	20
Boris Godounow	Sorrow	17

But all these added to those previously counted would make only 622 correct choices in the second discrimination, or 49 percent of the total, and the conclusion is unchanged that only in the first discrimination can we expect to find a majority of correct placements.

To what extent is the ability of gauging correctly the meaning of a composition dependent on musical training? In order to answer this question a further analysis of the data was made. Each auditor had on separate occasions filled out questionnaires concerning his education in music, and on the basis of this information a musical training score was computed, one point being allowed for each year of public school music, choir, glee club, band, orchestra experience, or musical appreciation courses, and four points for each year of private lessons or courses in theory. The scores ranged from 0 to 68, and the auditors were divided into three groups.

	Range of Scores	Number
Those with high training	17–68	23
Those with less training	10–15	16
Those with a minimum of training	0–9	33

The high and the low groups were then compared with the following results.

TABLE III

COMPARISON OF AUDITORS TRAINED IN MUSIC WITH THOSE UNTRAINED

	Percentages of Correct Discriminations		
	First Discrimination	Second Discrimination	Third Discrimination
Low group.....	71%	39%	24%
High group.....	75%	43%	26%

The difference between the groups is too small to be reliable, and we may conclude that training makes practically no difference in the ability to judge the emotional meaning of a selection. This result is in agreement with a similar conclusion made by Hevner [2] to the effect that trained musicians are able to judge the emotional significance of the major and minor modes only slightly better than untrained observers.

Summary.—College students listening to Victrola records can tell whether the music is intended to be sad or joyful, but when progressively finer discriminations are attempted, they are progressively less successful. Subjects trained in music do practically no better than those untrained. There is consequently little justification for the assertion that any certain composition is exactly right to express farewell, to represent the early morning, etc. Such feelings, when they do occur, would seem to be the result of association, and are more or less individual. Symphonic program notes, in so far as they attempt to give to the music any such inner appropriateness to particular moods or events, are without validity, although these notes may be instrumental in establishing widespread associations.

(Manuscript received January 12, 1937)

[2] Hevner, K., The affective character of the major and minor modes in music. *Amer. J. Psychol.*, 1935, 47, 103–118.

CAN THE BRAINEDNESS FACTOR RIGHT OR LEFT BE ESTABLISHED BY GRAPHIC TIME DATA ALONE?

BY CLARENCE QUINAN

On scrutinizing one by one the samples of bi-manual handwriting [1] obtained from 100 unselected normal persons—84 dextrals and 16 sinistrals—it was noted as to any given specimen that the two bits of writing it exhibited differed greatly from one another as to length. By means of this length-factor, therefore, it was a simple matter to separate the sample-cards into two lots. This having been done, exact linage data referable to the model test word employed showed that 71.66 percent of the dextrals had produced the larger linage values with the left hand, while 90 percent of the sinistrals had produced the larger values with the right hand. Script set down in this fashion sprawled along its base-line; and the characters were large, coarse and primitive in type. Seemingly, moreover, in a rough, linear sense the brainedness factor revealed itself either as a right or a left type of isomorphic bi-manual sprawl (1).

It was surmised that this peculiar sprawl phenomenon took place because two cerebral motor discharges of unequal intensity (left and right, respectively) reacted upon one another and disorganized the handwriting movements. Hence if the factor which produced the sprawl reaction was the precise synchronization of the two cerebral motor discharges—left and right—it did not seem practicable to gain much if any additional information about them by studying further the product of their combined activity—*i.e.*, bi-manual handwriting. However, even though in a roundabout manner, it did seem worth while, through the medium of uni-manual graphic tests, to study the motor discharges by meas-

[1] Bi-manual writings are produced by writing with the right hand and the left hand simultaneously, each hand moving in the direction away from the mid-line of the body.

uring separately in terms of exact time values the kinetic activities of the two hemispheres.

The object of this article is to describe briefly the results obtained in such a research.

The experimental data about to be set forth seem to show: 1, that any literate person can write with each hand in the direction either dextrad or sinistrad; 2, that an accurate and trustworthy time value[2] is to be had for each of the four handwriting activities thus possible; 3, that if but a single speed test be made daily as regards respectively the three unusual graphic movements (*i.e.*, one with the right hand, two with the left hand) that any series of such values—right or left—exhibits a striking phenomenon of progressive acceleration; 4, that on or about the third day dextrals begin to throw off the smaller time values with the right hand (left hemisphere) while on the contrary sinistrals produce the smaller time values with the left hand (right hemisphere); 5, that the habit of writing exclusively with the right hand does not in the least modify a sinistral's brainedness type.

Besides the invaluable quality of objectivity the aforementioned graphic time data seem to possess also the quality of constancy in that, quite unlike the notoriously variable 'handedness'-factor, each time value has to do presumably with but a single hemisphere—be it the left or the right.

Method

On 25 consecutive experiment days timed specimens of handwriting were obtained from four university men—two dextrals and two sinistrals. Of the two latter persons both are left-handers of the type 'LL'; both write exclusively with the right hand.

From each person tested four script samples were obtained, designated, respectively, RON, RIR, LOR and LIN. In these formulas the first letter is that of the hand, the second, indicates the direction of the movement, whether out or in (*i.e.*, centrifugal or centripetal) while the third letter signifies as regards the orientation of the text characters whether they face normally (to the right) or are reversed.

The time values were measured with a stop-watch, using as a model unit of work this sentence: "He protested that he couldn't eat the twenty-two tarts; and the truth is that he ate twenty." Each person examined produced his writing specimens at one sitting and in the following order: 1, with the right hand dextrad (RON); 2, with the right hand sinistrad (RIR); 3, with the left hand sinistrad (LOR); 4, with the left hand

[2] This seemingly definite, kinetic value is obtained by means of a stop-watch and a model sentence. Hence the figure thus secured is a graphic velocity-factor expressed in terms of seconds with reference to a constant unit of graphic work.

dextrad (LIN). In this article, however, only the data respectively of the movements RIR and LOR are considered.

A single round of measurements was carried out once daily with each person. None of the reactors indulged in writing practice.

Results

On referring to Table I it will be noted that the 200 exact time values shown there are arranged in two panels of equal size, separated by a median interval.

Now, depending upon the point of view, the eight columns of figures displayed in this table illustrate two different phenomena.

TABLE I

Quantitative Time Measurements of Mirror-writings

The values shown are in seconds, obtained by means of a stop-watch and a mode sentence. Comparison of dextrals and sinistrals as regards the relative time values produced respectively with the left hand and the right hand.

Day	Dextrals				Median	Sinistrals			
	H.D.		D.S.			T.A.		N.W.	
	L	R	L	R		L	R	L	R
1	164	144	98	66		48	69	51	64
2	139	124	100	61		41	55	49	55
3	127	104	100	66		40	55	49	57
4	132	103	85	61		34	47	42	59
5	106	91	84	58		34	44	41	52
6	105	89	76	55		34	44	40	47
7	98	83	62	50		31	41	38	51
8	90	77	66	52		31	43	34	46
9	94	77	60	50		29	39	33	45
10	98	75	57	49		31	42	35	47
11	95	74	58	45		30	38	32	46
12	85	74	60	41		30	37	29	42
13	91	70	49	41		30	42	30	42
14	88	68	53	40		29	40	28	39
15	80	69	49	39		28	35	28	45
16	80	64	57	36		30	34	28	43
17	80	64	48	38		28	32	28	40
18	79	63	45	36		27	31	25	37
19	72	62	47	32		26	30	27	41
20	74	63	43	29		26	30	27	33
21	74	58	38	28		25	28	25	32
22	73	60	35	25		26	28	26	32
23	75	60	34	27		23	27	28	35
24	73	62	35	26		24	29	27	36
25	75	60	38	25		24	28	26	33

Thus if they are examined in the vertical direction, from above downward, it will be obvious that the values become smaller at a fairly regular rate; in other words the graphic velocity-factor manifests a process of acceleration. The surprising thing about this stepping-up of the speed rate is that it occurred in every instance and despite the fact that no writing practice whatever had been indulged in. Indeed by the tenth day each person tested had stated voluntarily his belief that he had reached his utmost limit of speed; that it was useless to pursue the work any further; that he had 'absolutely touched bedrock.' Yet over and over again thereafter, to his intense amazement, he continued to make substantial gains. On being bent into a new path, therefore, seemingly the cerebral motor dicharges tend to traverse it with ever increasing speed.

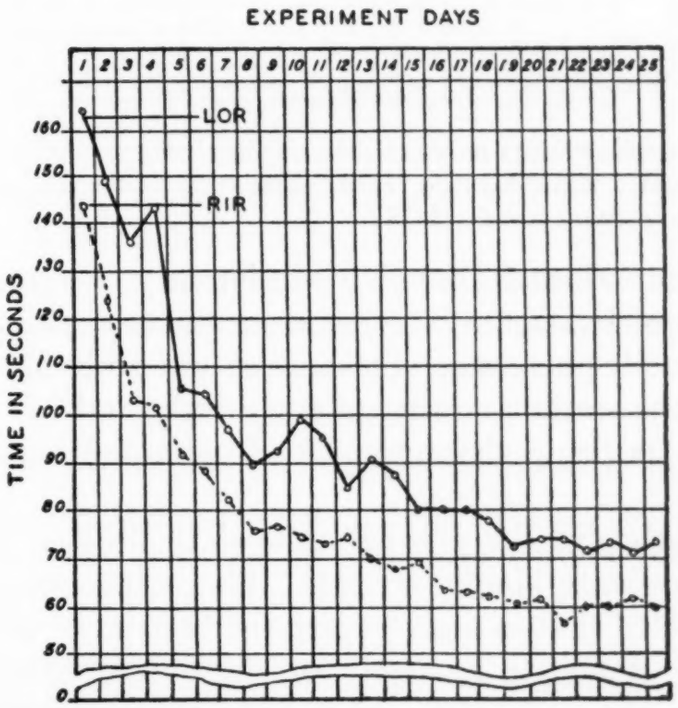


FIG. 1. Graphic velocity record of H.D., a dextral reactor. 'RIR'—time curve of the right-handed mirror-writings; 'LOR'—time curve of the left-handed mirror-writings. The dominant curve is 'RIR.'

If now the two panels of figures—those of the dextrals and those of the sinistrals respectively—are compared line by line, horizontally, it will be observed that the time values

of each dextral person diminish toward the right while on the contrary those of each sinistral person diminish toward the left. Consequently, since each panel—the dextral and the sinistral—faces toward the median line and therefore toward the opposite panel, the two panels exhibit as it were a sort of isomorphism.

The time values L and R of the reactor H.D., a dextral, are shown in Fig. 1 while those of T.A., a sinistral, are displayed in Fig. 2. In neither figure do the time curves overlap one

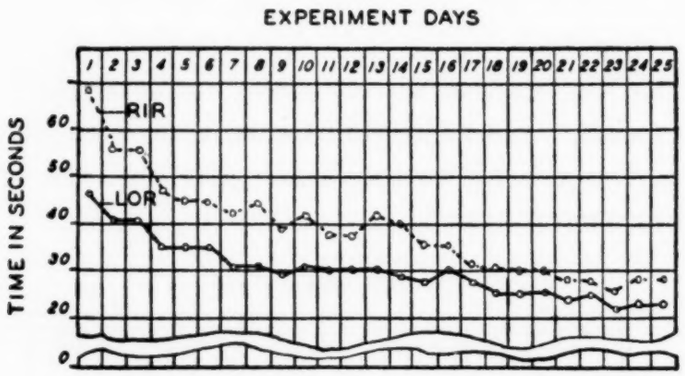


FIG. 2. Graphic velocity record of T.A., a sinistral reactor. 'RIR'—time curve of the right-handed mirror-writings; 'LOR'—time curve of the left-handed mirror-writings. The dominant curve is 'LOR.'

another, the values RIR being consistently the smaller ones in the case of H.D., the larger ones in the case of T.A.

This kinetic phenomenon resembles and probably is closely related to the static laterality effects—right and left—already referred to as being characteristic of bimanual handwritings (1).

How early did the graphic time data display these two seemingly definite kinetic trends? The point cannot be too strongly stated that in a given case the first set of time values recorded is of no use by itself for the determination of the brainedness type. For I found in numerous collateral experiments that a majority of persons—dextrals as well as sinistrals—made much better progress at first with the left hand than with the right. But on repeating the tests once daily it was noted usually on or about the third day that one hemisphere was far superior to the other hemisphere in the kinetic sense; indeed, often in that short space of time the speed data

changed enormously. For example, to cite a typical case, in one instance the value for the right hand fell from 230 seconds to 99 seconds in three days; but since during the same period the record for the left hand changed only from 225 seconds to 116 seconds, and since the values for the left hand continued to be the larger ones, thereafter, no doubt could be entertained that the person under examination was a true dextral—*i.e.*, left-brained.

Summary

A kinetic method for the determination of the brainedness factor is described the essential working element of which consists of a quantitative time value measured with reference to a constant unit of graphic work.

The method depends upon the principles:

1. That any literate person can write with each hand in the direction either dextrad or sinistrad;
2. That the time values noted as regards any person's mirror-writings become progressively smaller from day to day;
3. That the two mirror-writing time values—left and right—obtained respectively from a dextral person and a sinistral person exhibit a remarkable phenomenon of isomorphism—right or left as the case may be. This phenomenon usually manifests itself clearly on or about the third day.

Conclusions

1. Objective data are advanced which seem to show that a person's brainedness type can be established by graphic time data alone; that this object can be accomplished without the reactor's knowledge and without asking him a single question.
2. The brainedness factor does not appear to be modified in the least by a change of handedness.

(Manuscript received February 23, 1937)

References

1. Quinan, C., Involuntary graphic sprawl: a motor phenomenon related to mirror-writing and modified by the factor of handedness, *Arch. Neurol. & Psychiat.*, 1935, **34**, 318–329.

DATA BEARING ON THE CONDITIONING OF VOLUNTARY REACTIONS

BY J. M. STEPHENS

Department of Education, The Johns Hopkins University

The data [1] to be reported are offered as an extension or adaptation of a previous experiment (3). That experiment was designed for the purpose of investigating the direction of conditioning. Apparently, however, other people considered it as a potential answer to a quite different question, namely, "Can voluntary reactions be conditioned?" (CVR). At least Gibson (1) criticized the experiment for failing to answer that question completely. If the experiment is to be considered with regard to its bearing on CVR it should have, as Gibson pointed out, an additional control, simpler than the one I used. Such a control is presented here.

The reader should be warned, incidentally, that this control is in no way intended to bolster up the previous experiment as far as the original purpose is concerned. The data in this control are not necessary for the original problem, and will add nothing to its solution.

It seems necessary to point out also that I am not an investigator of CVR. I am merely providing data to make a previous experiment more useful to those who wish to use it for the extraneous purpose of investigating the conditioning of voluntary reactions. I am glad to supply this control, since I can do it more conveniently than anyone else, but in so doing I am not committed as to the value or originality of the extraneous problem (CVR) nor as to the adequacy of this approach.[2]

[1] I am indebted to Mr. Sprol and Mr. Aiosa, students in the Department of Education, for assistance in securing the records.

[2] Such a clarification of my position seems necessary in view of Mr. Razran's reference to me in his discussion (2) of the previous work on CVR. If Mr. Razran had understood my position as indicated here his reference to me would have been ridiculous. The fact that he made such a reference convinces me that my position was not clear.

I admit that he would also be justified in quoting me if my statement claimed or implied that the problem had not been investigated before. But this interpretation of my statement is ridiculous on its face, since the statement merely (*a*) agreed with Gibson that a given control is necessary for the demonstration of CVR and (*b*) offered to supply that control, "so that my data will have more bearing on that particular problem." The agreement says nothing more about the existence of previous work on the demonstration of CVR than the statement "Valves are necessary for steam engines," says about the existence of previous steam engines. The promise to supply the necessary control implies nothing further except that there may still be aspects of the problem concerning which pertinent data will be useful.

Procedure

For the basic control group, the experimental conditions were identical with those described in the previous study (**3**, pp. 82–84) except (*a*) that the subjects were college students of both sexes instead of boys from 14 to 16, (*b*) that nothing was said about the relative importance of the two tasks and (*c*) that no paired presentations were given. In the case of this basic control group the subjects sat expecting signals for either hand. At the times when the schedule of presentations called for a paired presentation, nothing was given. When the schedule called for a trial (a signal for one hand only) that was given.

In addition to the basic control, other conditions were arranged. It is possible that in the original experiment the interpolated paired presentations, in addition to eliciting certain responses, may have also kept the subject's mind off the trials. They may have kept him from rehearsing his move for a signal to the left or to the right hand. Now in the basic control there was nothing to keep him from such rehearsing. Consequently an additional group was secured. During the experiment an assistant read passages from the CAVD intelligence tests to the subjects of this group and quizzed them on the contents. No attempt was made to adjust the reading to the signals. The assistant ignored the signals and the experimenter gave the signals at the appointed time irrespective of the reading.

In a second modification visual instead of auditory signals were used. Here both conditions of no distraction and of distraction occurred. In the case of the visual signals a further condition was arranged in which there was no extrinsic

distraction but the trials followed one another very rapidly so as to prevent any elaborate rehearsing.

The results are given in Table 1.

TABLE 1

NUMBER OF SUBJECTS MAKING SPECIFIED NUMBER OF FALSE REACTIONS (FR) FOR VARIOUS EXPERIMENTAL CONDITIONS

No. of False Reactions	Auditory Signals				Visual Signals					
	Long Intervals				Long Intervals				Short Intervals	
	No Distraction*		Distraction		No Distraction		Distraction		No Distraction	
	Left	Right	Left	Right	Left	Right	Left	Right	Left	Right
5			1	1						
4		1								
3			1							
2	1	2	1	5	3		1		1	1
1	2	9	5	2	4	2	4	3	2	6
0	9		2	2	3	8	6	8	9	5
N (Subjects)	12	12	10	10	10	10	11	11	12	12
No. of FR	4	17	15	17	10	2	6	3	4	8
No. of trials	129	131	115	115	103	107	83	86	210	201
Percent FR	3	13	13	15	10	2	7	3	2	4

* Basic control.

The table consists of 10 frequency distributions. Thus from the first distribution we learn that in the case of signals to the left hand, one subject made as many as two false reactions, two subjects made one false reaction, and nine made no false reactions.

The last row of figures in the table reveals that in no case do we find false reactions in more than 15 percent of the trials. In the original study in which paired presentations were interpolated between trials, we found the false reactions in no case to be less than 32 percent of the trials. Unless this difference is due to one or more of the extrinsic discrepancies (such as age of subjects) between the groups, it would seem to be a result of the paired presentations which formed one factor in the first study.

Owing to the probable unreliability of the values, any comparison of one type of control with another would hardly

be profitable. The various groups were set up, not to compare one with another, but to try to insure that at least one of them would function as a valid control.

(Manuscript received January 22, 1937)

References

1. Gibson, J. J., A note on the conditioning of voluntary reactions, *J. Exper. Psychol.*, 1936, **19**, 397–399.
2. Razran, G. H. S., The conditioning of voluntary reactions, *J. Exper. Psychol.*, 1936, **19**, 653.
3. Stephens, J. M., The conditioned reflex as the explanation of habit formation; III. The operation of two higher-order reactions in close succession, *J. Exper. Psychol.*, 1936, **19**, 77–90.

A MULTIPLE-EXPOSURE TACHISTOSCOPE

BY C. E. FERREE AND GERTRUDE RAND

From the Research Laboratory of Physiological Optics, Baltimore, Md.

A multiple-exposure tachistoscope has many uses. The instrument to be described in this paper was devised by us to be used especially for measuring the dynamic speed of vision, speed of accommodation, and ocular fatigue and recovery. This instrument has been used very successfully for these purposes by Lieutenant-Commander C. J. Robertson of the U. S. Naval Hospital in his recent studies of aviators and their classification by test as to fitness of performance.[1]

There are two types of test for speed of vision: a static test and a dynamic test. In the static test the eyes fixate and accommodate for a given selected distance and the test object is exposed at that distance for the minimum time needed for its discrimination. Such conditions test only the speed of reaction of the sensorium. They give no information as to the motor functions of the eyes and the effectiveness of their coordination for the purpose of seeing. In the dynamic test for speed of vision, on the other hand, the eyes are required to shift their regard from one object to another or to a series of objects and to discriminate them in turn during the time or times of exposure. These conditions test not only the speed of reaction of the sensorium but also the oculomotor facility and efficiency.

So far as the discrimination of an object is concerned, the static test is sensitive for such purposes as a determination of the effect of intensity or color of light, or in general of such factors as affect acuity and visibility. It can also be used for

[1] C. J. Robertson, Measurement of the speed of adjustment of the eye to near and far vision, *United States Naval Medical Bull.*, 1934, **32**, 275–283; A comparative study of the measurement of the speed of adjustment of the eye for near and far vision, *Ibid*, 1935, **33**, 187–205; Measurement of speed of adjustment of eye to near and far vision, *Arch. Ophthal.*, 1935, **14**, 82–89; Measurement of speed of adjustment of eye to near and far vision. A further study, *Ibid.*, 1936, **15**, 423–434. Further papers by Dr. Robertson are in press.

determining the time threshold of sensation, the course of the rise of sensation from minimum to maximum, and for making other sensory determinations in which time of exposure is the important factor. The dynamic test, in addition to testing oculomotor facility and efficiency, is of value for determining individual fitness for different types of work; and is especially valuable for studying in a given individual general and ocular fatigue and recovery as well as all conditions, ocular and general including pathologic disturbances, that would affect muscular coordination and efficiency. The various uses and applications of this test will be discussed in greater detail later in the paper.

We have devised instruments for making both the static and the dynamic test. Work in which the former instrument was used has been described in previous papers.[2] Our purpose here is to describe the instrument we have used for making the dynamic test.

Description of Instrument

This instrument has in general the following features: two near test objects suitably located on either side of the median plane of the observer and one far test object located in the median plane, all three of which are at the same vertical level and have a provision for an objective check on the judgment; an apparatus for the exposure of the test objects in the serial order: near, far and near; a means of driving the apparatus at suitable rates of speed; and a provision for the remote control of the far test object from the position of the examiner.

[2] C. E. Ferree and G. Rand. The effect of variations in intensity of illumination on acuity, speed of discrimination, speed of accommodation and other important eye functions, *Trans. Illum. Eng. Soc.*, 1920, **15**, 769–792; The effect of variation of visual angle, intensity and composition of light on important ocular functions, *Ibid*, 1922, **17**, 69–86; Further studies on the effect of composition of light on important ocular functions, *Ibid*, 1924, **19**, 424–447; Flicker photometry and the lag of visual sensation. *Amer. J. Psychol.*, 1924, **35**, 208–216; Intensity of light and speed of vision. I, *J. Exper. Psychol.*, 1929, **12**, 363–391; II, *Ibid*, 1930, **13**, 388–422; Size of objects in relation to their visibility and to the rating of vision, *Arch. Ophthal.*, 1930, **4**, 37–72; Visibility of objects as affected by color and composition of light. I, *Personnel J.*, 1931, **9**, 475–492; II, *Ibid.*, 1931, **10**, 108–124; The problem of late afternoon lighting, *Arch. Ophthal.*, 1932, **7**, 558–575.

A view of the instrument intended to represent only the general test situation is given in Fig. 1. This illustration

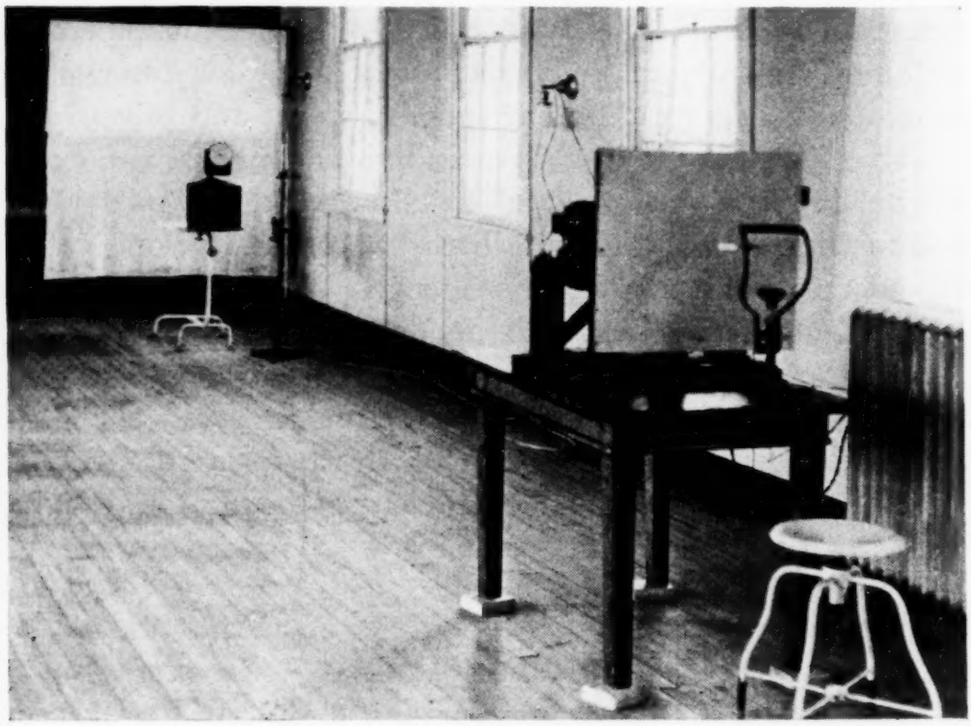


FIG. 1. A view of the instrument designed to represent the general test situation.

shows the head-rest at the position of the observer, the front of the exposure apparatus and the far test object, but gives none of the details of construction of the instrument. Fig. 2 is a close-up view of the exposure apparatus which shows the back of the near test cards, the driving mechanism and the exposure disks so positioned in their circle of rotation as to give the details of their construction.

The exposure apparatus might be called a multiple-exposure tachistoscope, compounded to meet the needs of the test. It is furnished with four disks located in front of the near test objects and as near as possible to them:—a pair of large disks *A* and *D*, a single disk of smaller diameter *B* and a single disk of intermediate diameter *C*. These disks were cut from hard sheet aluminum, No. 20 B. & S. Gauge, as shown in Fig. 2. The inner portion, radius 6.5 cm, is continuous through 360 degrees and the outer portion extends through 180 degrees. The breadth of the outer zone of disks *A* and *D*

is 22.5 cm; of *C*, 20.5 cm; and of *B*, 14.5 cm. Disk *A* is permanently pinned to the axle; *B*, *C* and *D* are adjustable.

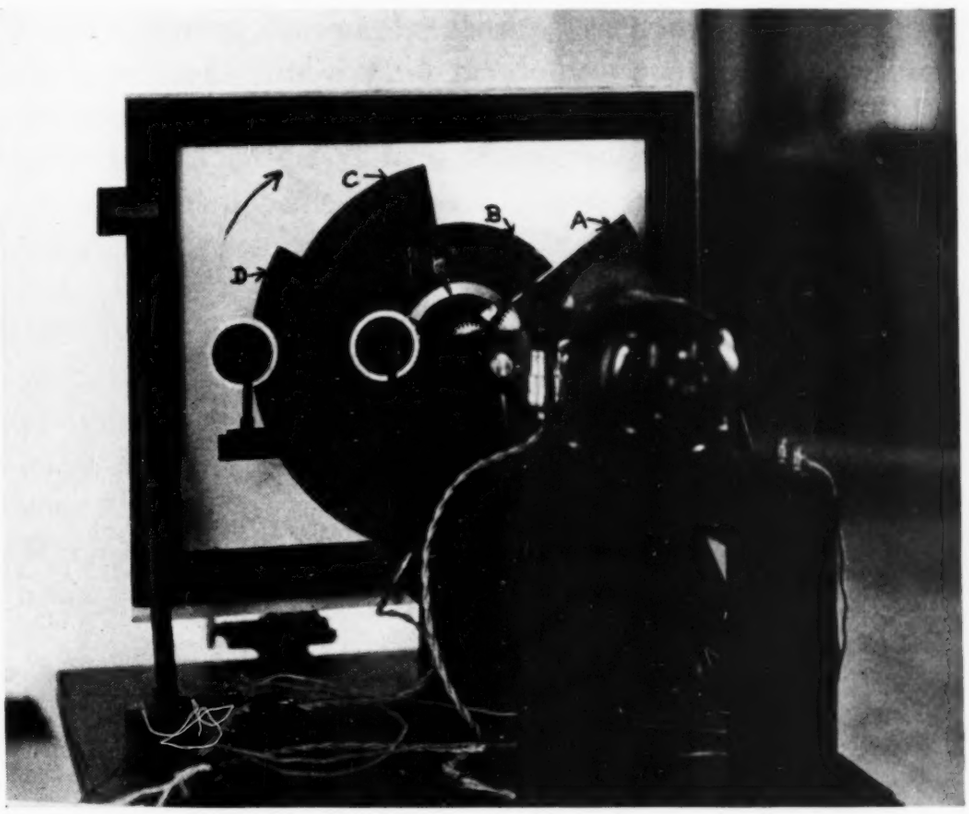


FIG. 2. Close-up of exposure apparatus, showing the driving mechanism, the backs of the near test cards and the exposure disks so positioned in their circle of rotation as to give the details of their construction.

The diameter of disks *A* and *D*, when in position, is sufficient to cover all three test objects—the two near test objects one on the right and one on the left of the median plane of the observer and the far test object in the median plane; that of disk *B*, to cover only the near test object on the left; and that of disk *C*, to cover the far test object. When the disks are at rest prior to beginning the test, disk *A* covers the three test objects. On this disk directly in front of the near test object on the left is a cross for the control of fixation and the adjustment of accommodation. As the disks turn, this test object is exposed and then cut off from the observer's view by disk *B*. The eyes then diverge and swing to the right to see the far test object in the median plane. This exposure is ter-

minated by disk *C*. Continuing their excursion, the eyes converge and swing still further to the right to see the second near test object, the exposure of which is terminated by disk *D*. Thus the aperture between *A* and *B* provides for the exposure of the near test object on the left, that between *B* and *C* for the exposure of the far test object, and that between *C* and *D* for the exposure of the near test object on the right.

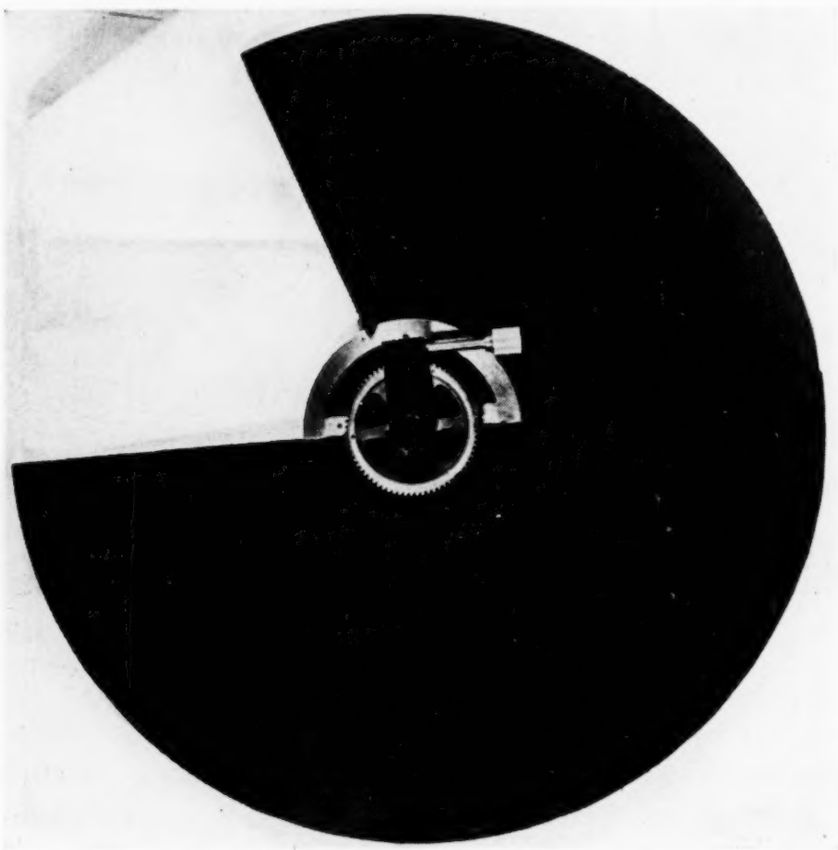


FIG. 3. Device for a quick and easy means of adjusting the movable sector in a sectored disk and holding it firmly in any given setting.

Disk *A* is provided with a graduated, semi-circular scale fastened along the edge of the inner portion of the disk as shown in Fig. 2; and disks *B*, *C* and *D* with raised narrow pointers having a straight edge flush with the radial edge of each disk, so positioned that when the disks are assembled on the axle, the pointers travel over the scale. On this scale are three sets of divisions—one in terms of degrees of open sector and the other two in units of time corresponding to the two

speeds of rotation of the disks provided for in the driving mechanism.

The disks are mounted on an axle which turns in bearings at the top of a heavy Y-shaped support. Disk *A*, as stated above, is pinned to the axle in such a position that when at rest before the beginning of the exposure it covers all three test objects. Disks *B*, *C* and *D* are free to turn about the axle and when adjusted for a given value of exposure are clamped in position by means of a nut and washer. This means of adjusting the position of the disks is the one inconvenient feature of the apparatus. Because of this inconvenience we have, since this model was constructed, devised a very quick and easy means of changing the value of the open sector of the disks. This device as applied to a single pair of disks is shown in Fig. 3. One of the disks is pinned to the axle. Also fastened to the axle is a bench or support for the worm-member of a worm-gear. The other disk is fastened to the cog-member of the worm-gear, this cog being free to turn in a groove about the axle. To the worm is attached a slender rod of suitable length fitted with a milled head. As the milled head is turned the worm engaging the cog rotates the movable disk. This device provides a quick means of both changing the value of open sector in continuous series and holding the movable disk firmly in any position in which it is turned. Obviously, with suitable and feasible modifications, gears of this type can be attached to operate each of the movable disks of the instrument described. With their addition extreme ease and simplicity are given to the operation of the instrument. Instead of having to loosen a nut, change the position of the disk by hand and again tighten the nut, all that is required is the turning of a milled head by the desired amount.

The driving mechanism seen in Fig. 2 consists of a motor of the synchronous type to give constant speed of turning the disks; a system of gears, worm and cog, for the reduction of speed of rotation of the disks; an intermittent gear of the cog type, the purpose of which will be described later; a gear-shift to change the speed of rotation; and an electric key for starting and stopping the motor.

The motor is wound to operate on a 60-cycle alternating current and to make 1800 revolutions per minute. The first worm-gear reduces the speed in a ratio of 40 : 1, or from 1800 to 45 revolutions per minute; a second reduces it in a ratio of 3 : 1, or from 45 to 15 revolutions per minute, *i.e.*, 1 revolution in four seconds. This gives the faster of the two speeds needed for making the test. A second speed of half this value is obtained with a cog-gear. By means of a gear-shift, the details of which need not be described here, either of the speeds just mentioned can be produced. With maximum breadth of open sector, the lengths of exposure given by the two speeds are 2 and 4 seconds. Intervals of exposure shorter than 2 seconds and between 2 and 4 seconds are obtained by suitable adjustments of the breadths of open sector. Since the speed of rotation is constant, one degree of open sector gives an exposure of 0.011 second at the faster rate of turning and 0.022 second at the slower rate.

The intermittent gear connects the axle which turns the disks to the driving shaft. The relation of the parts of this gear is such that the disks make one complete rotation, then stop with the edge of disk *A* always in the same position in the circle of rotation, namely, just covering the test objects. The driving shaft, however, continues to turn until stopped by the operator who starts the motor by pressing an electric key and stops it by releasing the key. By watching the intermittent gear he can stop the driving shaft in such a position that when the next exposure is made, a suitable interval will have elapsed between the starting of the motor and the beginning of rotation of the disks. A short interval is needed here both as preparation for the observer and to give the motor time to reach a constant speed.

Immediately behind disk *A* are two circular cards, 7 cm in diameter, along the edge of which are printed the near test objects. The test object chosen was the letter *E*. These *E*'s, eight in number, are so positioned that by rotating the card they can be presented precisely at the same place, pointing up, down, right, left or in the four 45-degree positions, in any order chosen. At a corresponding position of the back

of the cards are printed designations indicating the direction in which the exposed letter points. Each of the cards is mounted at its center on a small metal disk at the end of a grooved pin 6.5 cm long. This pin passes through a collar provided with a set-screw, which feature permits of a certain latitude of adjustment of the distance of the test card from the exposure disks. To provide for the rotation of these cards, the collar turns in a sleeve supported by a grooved carrier which slides on a track to permit of the needed latitude of adjustment of the test object from right to left. In order that the test card when rotated shall come to rest in each of the positions indicated above, eight stops are provided constructed as follows. Just back of the collar already noted and in contact with the sleeve is a shoulder of the same diameter as the sleeve. The front wall of this shoulder contains eight very shallow cups, the centers of which are spaced in a circle about the axis of rotation in the same radial planes as the centers of the test objects. In the adjacent wall of the sleeve at the same distance from the axis of rotation as the cups, is a small socket of suitable depth. In this socket is a coiled spring, in the front end of which is embedded a small ball. This ball engages the cups as they pass in rotation. For convenience of rotation in the sleeve the collar is provided with a milled head, and to permit of smoothness and freedom of movement in turning, the circle containing the cups is grooved.

Between the observer and the exposure disks and as near to the latter as possible is a cardboard screen with an aperture of such a height and breadth as to give a clear view of the near and far test objects with each eye but to exclude the rest of the field of view. The near test cards are illuminated by well diffused light reflected from the mat surface of the back of the screen. The light is supplied by a tubular tungsten lamp enclosed in a cylindric housing provided with a vertical aperture of suitable breadth, which can be rotated about the lamp to give the desired angle of incidence of light on the screen and the desired amount of light reflected to the test surface.

The task set the observer by the near test objects may be

varied to suit the purpose of the text by suitable changes in either the size or the distance of these test objects or in both. This feature requires special consideration in the use of the instrument in making a limiting test for age with reference to fitness for different types of work. This test will be discussed later in the paper.

The far test object, which is also the letter *E*, is printed on a small circular card, 3 cm in diameter, mounted on a metal disk of the same diameter at the end of a short pin, which is inserted into a shaft and secured by means of a set-screw. The shaft is connected by gears to a small reversible motor which turns the test object into the eight positions selected to provide an objective check on the judgment: up, down, right, left and the four 45-degree positions. The motor is wound for constant speed and the direction of rotation can be reversed at will by a switch. The stopping of the test object always in the same direction in the eight positions is accomplished by means of an intermittent gear of the Geneva type. The relation of the parts of this gear is such that the test object turns and stops in sequence as long as the motor is running. The interval of stopping is long enough for the operator to discriminate the test object and judge whether the position attained is the one desired, but not too long to interfere with expedition in making the test. The motor is started by pressing an electric key at the position of the operator and stopped by releasing the key. If the operator wishes to reverse the direction of rotation of the test object the key of the reversing switch is pressed. Thus the greatest possible latitude is afforded for the adjustment of the position of the test object in haphazard order.

The speed of rotation of the motor is 3000 cycles per minute. This speed is reduced in the ratio of 80 : 1 or to 37.5 cycles per minute, by means of a worm-gear connected to the shaft which turns the test object. The motor and gears are housed in an oblong box of suitable size mounted on a small table at a distance of 6 meters from the observer or at any other distance desired.

Surrounding the small metal disk to which the test surface

is attached is a larger disk, 12 cm in diameter, painted mat white. This furnishes the more remote surrounding field for the test object and is surfaced to give the same reflection as the small circular card on which the test object is printed. A hole is cut in this field just large enough to receive the test card. The test object thus turns, but the field is stationary, the fitting of the test card and field being so close than an unbroken surface is seen by the observer in position at the exposure apparatus. The test surface is illuminated from in front to any brightness desired. The far test object and the near test objects should have the same intensity of illumination. The adjustment of these intensities can be made most conveniently by measuring the brightness of the far test card and matching that of the near test cards to it by viewing the near and far test surfaces in quick succession from the position of the observer at the exposure apparatus. A unit which we have recently devised for varying intensity over a wide range is excellent for the illumination of the far object.[3] It can be mounted on floor stand, or a smaller unit of the same type can be mounted on a bracket support fastened to the table. In these units the intensity of light can be changed in continuous series from zero to full value by a simple mechanical device, without change in the color or composition of the light.

A convenient type of head-rest for use with the instrument is shown in Fig. 1. This head-rest is securely fastened to the table on which the exposure apparatus is supported. Three adjustments are provided, which are operated by coarse-threaded screws having a suitably fast motion:—up and down, right and left, and backward and forward. By means of these adjustments the observer's eyes can quickly be brought into the required position. The height of the supporting table is also adjustable.

The instrument is easy and convenient to use. As has already been indicated, its manual operation requires nothing more than the pressing and releasing of an electric key and, in the preferred form, the adjustment of the disks by turning

[3] C. E. Ferree and G. Rand. Needs and uses of variable illumination and a convenient device for obtaining it. *J. General Psych.*, 1936, 14, 473-487.

a milled head. We feel that it can be confidently recommended as entirely feasible for practical work. As shown, the instrument is only a laboratory model. Designed for practical use, the exposure apparatus can be made much smaller and neater, and both it and the far test object can be mounted on floor stands.

Test Procedure

The three test objects and the eyes of the observer are adjusted to the same vertical level, the near test cards being separated enough to give the observer a clear view of the far test object with each eye. Disk *B* is adjusted to give a length of exposure that will just permit of discrimination of the near test object on the left. Disk *C* is adjusted to give a length of exposure that will just permit of discrimination of the far test object and the change of adjustment of the eyes needed for this discrimination. Disk *D* is adjusted so as just to permit discrimination of the near test object on the right and the change of adjustment required for this discrimination. If it should be desired to measure the time required for the passage from near to far and back again to near in continuous change, disk *C* is turned back out of the way, and the adjustment for the double excursion is made with disk *D*. While the measurement can be made more quickly by this procedure than by the first, it has the following disadvantages: (*a*) A separate determination is not obtained of the time required for the two halves of the excursion, from near to far and from far to near. This separation may be of importance in testing in clinics and in other types of testing. (*b*) No provision is made for preventing the eyes from lingering on the far object before beginning the excursion toward the near object. The interval required for the double excursion as measured directly by this procedure, for example, is appreciably longer than when it is determined by adding the values of the two halves as measured by the first procedure. This tendency of the eyes to linger when they can thus not only makes a difference in the absolute value of the results, but it may also affect comparative values. That is, not only does the latitude offered give a chance for a variable performance or a variable

error from time to time with the same observer, but it leaves the results open to the influence of this factor in the case of different observers. It may be, on the other hand, that individual differences in the laziness of the eyes are an important factor in the rating of persons as to vocational fitness and that the second procedure is therefore better fitted for this purpose than the first.

It should perhaps be noted that the adjustment of disk *B* is not made primarily for determining speed of perception with the eyes in approximate adjustment for the near object, although this is an item that might be of value in a comparative study of the lag of ocular functions in different persons. It is made chiefly in order that the determination of the time to pass from near to far may be made with greater precision. That is, if the observer is to begin his excursion from near to far with an exact adjustment of clear seeing at near, he must be required as a check to start with a task which involves a report of clear seeing at near. The mere instruction to fixate a cross or other object will not guarantee the needed adjustment. Moreover, the adjustment must be precisely controlled if the results are to be safeguarded against the variable error already discussed. If it is not controlled, the eyes may linger too long at near or begin their change toward far too soon and the amount of the deviation in either direction may vary from time to time and from person to person.

In making the test, either the oculomotor or the accommodative reaction may be emphasized. To emphasize the part the oculomotor efficiency plays in the test, the range of lateral excursion of the eyes should be increased. This can be accomplished by moving the near test objects as far as possible to the right and left of the median plane. If need be, the breadth of the exposure disks may be increased. Also, the far test object may be given other locations than the median plane and, if desired, its distance from the near objects can be decreased. The size of the image of the retina for objects subtending a visual angle of 1 minute is of the order of thousandths of a millimeter. Great precision of coördination of action of the ocular muscles is required to superimpose

these images so perfectly as to cause no blurring due to doubling. That this can be done so quickly by the action of twelve muscles, each of which plays a part in every movement and all of which serve collectively to hold the eyes in position, is almost unbelievable. It could not be done at a medium or high rate of speed if the mechanism were structurally or functionally at fault. The realization of this is sufficient to indicate how effective a dynamic test of speed of vision should be as a check on the structural and functional perfection of the motor mechanism. This form of the test offers perhaps the greatest possibilities for usefulness in the work of the clinic.

If the object of the test is to measure speed of vision with a change of accommodation and as little emphasis as possible on the lateral excursion of the eyes, the test should be performed with one eye alone, and the near test objects should be brought as close as possible to the median plane, with just enough space left between them to enable the subject to view the far object. This relation of the test objects reduces the amount of ocular movement to a minimum, an amount that is perhaps negligible in the total performance. Again, measurement may be made of the time required to change from near to far and from far to near in separation or in combination. There is perhaps in this case a special interest in the separate determination of the two components of the adjustive process for distance, or what is often called accommodation and relaxation of accommodation.

Obviously, the test in any of its forms can be made in three ways: (*a*) As has just been described, the maximum performance for each person may be determined. This would be the analogue of making tests of visual acuity, for example, in terms of the minimum visual angle that can be discriminated. This procedure is the longest because it requires a correct adjustment of the disks for each observer, but it results in a much finer grading of performance. (*b*) Any suitable number of levels of performance may be chosen and the apparatus set at once to give these levels. This method of testing would place persons in ranks or groups and

is the analogue of the Snellen method of grading visual acuity. It is a quicker procedure than (*a*), but the grading is correspondingly rough. By a practised examiner, testing by this method should take but a few minutes. In forming such a graded scale in our preliminary work we have used steps of 0.2 second for the double excursion. These intervals have been found very suitable by Dr. Robertson in his work of classifying aviators as to fitness of performance. An alternate procedure would be to determine directly specific vocational scales made up of the number and breadth of steps needed for the various vocations. (*c*) Critical or limiting values could be established for any purpose for which such values are desired. The instrument could then be set for these values after the usual preliminary trials to familiarize the observer with the test, and the determination made whether he could give the judgments required, which could be done in one rotation of the exposure apparatus.

In the application of the test to work in clinics, critical values could be established as separating pathologic from nonpathologic conditions in persons of different ages. The test could then be made, as has already been indicated, with a single setting of the apparatus. Or the speeds could be measured for each person and the results compared either with a predetermined scale of norms or, as a check on progress, with a result obtained for that person at some other time.

Uses of the Instrument and Test

The test makes possible: (*a*) The use of a set of very sensitive tests which take into account as no other tests do both the motor and the sensory functions of the eyes in just the proportion that they occur in the act of seeing objects in different directions and at different distances.[4] Composite tests of this kind give by direct measurement information that cannot be obtained, and permit of conclusions that cannot be drawn, from any amount or kind of separate testing of functions.

(*b*) The testing of the dynamic speed of vision with either the oculomotor or the accommodative feature emphasized.

[4] See C. J. Robertson. Measurement of speed of adjustment of eye to near and far vision. A further study. *Arch. Ophthal.*, 1936, 15, 423–434.

The test made with the accommodative feature emphasized gives the speed of accommodation as nearly as possible, it would seem, for the result to be obtained. Similarly, the arrangement of the conditions of the test so as to emphasize the importance of convergence and divergence in the results measures the speed of these functions as nearly in complete separation as can be accomplished.

(*c*) The measurement of the time required to change from near to far and from far to near either in combination or separately.[4] The measurement of the time required for these shifts separately in the form of the test in which the oculomotor functions are emphasized gives a sensitive comparison of the facility and condition of the converging and diverging mechanisms, which may be more significant and comprehensive than tests of the duction type. Such measurements also provide a way of making a quantitative determination of what the results of the duction tests mean in terms of typical oculomotor performances. In the form of the test in which the accommodative feature is emphasized, the measurement of the time required for the shifts separately gives a comparison of the time required for the action of the muscles of accommodation and the lens material in the change from near to far and from far to near, which may have both a practical and a theoretical bearing. All the aforementioned possibilities of measurement have, we believe, great value in relation to the examination of the eyes in the work of the clinic.

In addition to these clinical applications, the following further uses of the instrument and the measurements it makes possible may be discussed:

1. A test of vocational fitness in all cases in which dynamic speed of vision is an important requirement. Most of the work requiring speed of vision is done with moving eyes. For such work a static test, which tests only the speed of reaction of the sensorium, is incomplete. A quickly reacting sensorium, it is true, gives a quick discrimination of the object and a quicker and more effective guidance of the movement of the eyes. However, it is not nearly so important for the

quick use of the eyes in the greater part of work that persons are called on to do as are quick oculomotor and accommodative adjustments. The dynamic test of speed of vision was devised by us during the World War as a test for fitness for aviation. We thought that the eyes of aviators should have a supernormal speed and precision of coordinated response in the three fundamental reactions: oculomotor, accommodative and sensory. Only the reaction of the sensorium is tested by static tests of speed of vision, and the important element of speed is entirely left out in all other types of ocular tests. The incompleteness and lack of special fitness of the usual and ophthalmological tests for this purpose should be obvious without further elaboration.[5]

2. A limiting test for age. As the eyes grow older the importance of accommodation in a dynamic test for speed of vision increases until it becomes the dominant factor in the result. Speed of accommodation is not an important factor in the results for the young eye unless there is some accommodative abnormality. Accommodation is the first of the ocular functions to deteriorate with age and the one that shows the closest correlation with age. The earliest manifestation of what would ordinarily be called a deterioration in this function, that is, a deterioration of significant practical importance, is, so far as measurements have been made, a decrease in speed. A comparative test of dynamic speed of vision of the eyes of the young and those of the middle-aged and near middle-aged furnishes striking and most conclusive evidence that the latter are under a special handicap in regard to all types of work that require speeds that are normal or supernormal for young eyes.[4] There are many vocations for which a high standard of requirement in this connection is needed. In a machine age, in which all industrial performances are quick and public and private conveyances travel at rates of speed that were formerly almost inconceivable, the demands put on the speed of the eyes' reactions have become increasingly rigorous. For example, many automobile drivers in an early stage of presbyopia have advised us that they dare not take their eyes off the road for

[5] See footnote 1.

the length of time they require even to read the speedometer. Driving under conditions which do not permit of the inspection of the instrument panel is, it will be granted, somewhat hazardous. Further, the requirements of factory workers who operate machines are by no means low. It is our strong belief that ocular tests in which the factor of speed is not given great importance are not a safeguard against either inefficiency of performance or dangerous accidents.

There is also, of course, an important need for a limiting test for age in aviation. The aviator in combat flying and in landing, particularly on water, needs everything that can be had in the way of dynamic speed of vision. In the early days of aviation—for example, during the World War—such a test was not so greatly needed as now. The vocation was new and by far the greater number of aspirants were young men. Now the aviators of that time, such as have survived, have grown older. It is difficult to make them realize that they may no longer be fit for the service or all branches of it and hard, without definite and concrete evidence of unfitness, to make a case against them that will stand.

The chief value of the test as a limiting test for age is in its application to persons between the years of 30 and 45 or 50, the period for which a precise and sensitive test is most needed and most difficult to obtain.

3. A means of measuring ocular fatigue and recovery and of testing individual susceptibility to fatigue and capacity to recover. The addition of speed or power to sustain to any test performance gives greater sensitivity for the detection of fatigue. For example, in a study of ocular and oculomotor fatigue, the test of dynamic speed of vision can be made at suitable intervals during the performance of a task, and from the results a curve for fatigue can be plotted for each person tested. From these curves individual susceptibility to fatigue of persons in the same age group and of those in different age groups can be compared. There are great individual differences in this susceptibility, and it is needless to point out that it is an important factor to be considered in determining ocular fitness for any type of work, especially in all

performances requiring a high degree of ocular fitness. Age, moreover, is a most important factor in this susceptibility. One would expect, therefore, that using the test of dynamic speed of vision as a fatigue test would be a most sensitive way of determining the effects of age on ocular fitness.

In addition to determining ocular fitness and susceptibility to ocular fatigue, the instrument and test may be used for testing the rate of recovery from ocular fatigue. From a study of the curves for ocular fatigue and recovery, persons of the same age group and those of different age groups can be compared as to susceptibility to ocular fatigue and the capacity to recover. A greater difference due to age might be expected for the latter than for the former. Our experience and observation is that while older persons become fatigued more easily, the especially marked handicap of age is on the side of recovery.

An instrument that furnishes a means of measuring and making a quantitative comparison of both ocular fatigue and recovery, in addition to measuring speed of performance, should have both important scientific interest and a practical utility over a wide range. Such a means would be of value not only in the selection of persons in all vocations in which special ocular fitness is required but in checking up on the persons selected for their ocular fitness at any time to accomplish an assigned task. A record of the dynamic speed of vision and the rate of ocular fatigue and recovery for any person gives a comprehensive estimate of his ocular fitness. In addition to the particular service rendered, studies comprising such determinations give information of general value.

In connection with the discussion of ocular fatigue and recovery it should be borne in mind that oculomotor fatigue and depression sustain a general relationship to general bodily fatigue. This relationship is sufficiently close to suggest that a fairly comprehensive estimate may be had of the bodily condition by testing the combined ocular and oculomotor performance. Just how much work has been done in direct confirmation of this sympathetic relationship

we are not prepared at this time to state. However, our attention has been called to experiments recently made in Soviet Russia, where a great deal of interest is being taken in physiological optics of the practical type we have been trying to develop. These experiments were conducted by J. J. Troumpaitz in the Laboratory for Lighting Research at the Leningrad Institute for Industrial Hygiene and Accident Prevention. The test of the ocular condition used was one devised by us for studying the effect of lighting conditions on ocular fatigue by measuring the eyes' power to sustain clear seeing for a period of time. Bodily fatigue was induced by the operation of a device called a bicycle-ergograph, which was selected for the study because its operation required no fatiguing use of the eyes. Nevertheless, a close relationship was shown to exist between bodily fatigue as measured by the ergograph and ocular fatigue as measured by the test of power to sustain clear seeing.[6]

Summary

The instrument described for measuring dynamic speed of vision, speed of accommodation and ocular fatigue is a multiple-exposure rotary tachistoscope comprising a driving mechanism, four sectored disks so arranged and of such sizes as to expose in immediate succession, in turning, a near test object on the left, a far test object in the median plane and a near test object on the right. The test objects are the letter *E*, the openings of which can be turned in eight different directions to give an objective check on the judgment. The far test object is provided with a remote control such that these adjustments can be made by pressing an electric key at the position of the examiner. The distance of the far test surface and the lateral separation of the two near test surfaces can be varied at will. The sectored disks are turned by means of a motor of constant speed provided with gears to give suitable reductions in speed and an intermittent gear which causes the disk covering the test objects prior to the

[6] From a report of work submitted personally to the authors by J. J. Troumpaitz. The purpose of the experiment was to show the loss of efficiency which the eyes sustained during a period of heavy physical work.

beginning of the exposure to stop in exactly the same position at the end of a single rotation. In the preferred form the values of open sector are adjusted by small worm-gears, which serve to change the position of the movable disks and to hold them firmly in position for any given setting. Exposure may be made in continuous series up to four seconds. Both the angular values of open sector and the times of exposure can be read from suitably positioned graduated scales.

The instrument makes possible: (*a*) the use of a set of very sensitive tests which take into account as no other tests do the motor and the sensory functions of the eyes in just the proportion that they occur in the act of seeing objects in different directions and at different distances, (*b*) the testing of the dynamic speed of vision with either the oculomotor or the accommodative feature emphasized and (*c*) the measurement of the time required to change from near to far and from far to near in combination or separately.

The following practical uses of the instrument and test are discussed: (*a*) as a means of detecting abnormalities and depressions in the oculomotor functions in the work of the clinic, (*b*) as a test for vocational fitness in all cases in which dynamic speed of vision is an important requirement, (*c*) as a limiting test for age, (*d*) as a means of measuring ocular and oculomotor fatigue and recovery and of testing individual susceptibility to ocular fatigue and the capacity to recover, and (*e*) as a means of training eyes to greater oculomotor and accommodative facility.

As a test providing one or more continuously variable and accurately timed exposures, the instrument has of course many uses besides those discussed in the paper.

(Manuscript received January 29, 1937)